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ABSTRACT

This book discusses modern scientific inquiry and examines the procedures of inquiry into human behavior used in the behavioral science disciplines. Psychologists look at the individual's adjustive procedures and the evolution of those adjustments within a species. Anthropologists inquire into the behavioral similarities and differences of human cultural groupings, from earliest man to the present. Sociologists investigate the behavior of people in groups and organizations, including the patterns, regularities, variations, and developmental changes in human relations, customs, and institutions. Political scientists study the behavior of human individuals and groups, with an emphasis on the distribution and attainment of political influence and power and on the function, organization, and connections among the political units, institutions, laws, and customs. Other disciplines examined in the book are economics, history, jurisprudence, linguistics, game and decision theory, information theory and cybernetics, and general systems theory. The book devotes a chapter to each field. In addition to the discussion, a selected bibliography and a listing of germane journals are provided for each discipline. (Author/RM)

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A CURRENT APPRAISAL OF THE BEHAVIORAL SCIENCES

REVISED EDITION

A CURRENT APPRAISAL
# of the
Behavioral Sciences

Revised Edition by

ROLLO HANDY and E. C. HARWOOD

First Edition by

ROLLO HANDY and PAUL KURTZ

BEHAVIORAL RESEARCH COUNCIL
Great Barrington, Massachusetts

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PREFACE

THE Behavioral Research Council is a private nonprofit research and educational institution designed to promote and to carry on research into the problems of men in society. Its aim is to supplement rather than duplicate the work of existing agencies.

A canvass of current agencies showed that such supplementing seems most needed on long-term and large-scale social research that requires and would develop the most useful interdisciplinary methods and the most talented researchers to direct such inquiry. The BRC therefore seeks to supplement the present agencies: (1) through gathering funds and resources on a massive scale eventually, however modest at the start; (2) through thorough research training of talented youth who would be well supported for such complex inquiry; (3) through far-reaching research projects commensurate with man's major problems, carried through to publication of scientifically warranted information on the probable costs and consequences of alternative courses of action. The very remoteness of these goals makes it the more urgent that some supplementing agency begin work along these lines promptly.

The authors of the first edition of *A Current Appraisal of the Behavioral Sciences* undertook the extensive investigations and writing involved. Their first drafts were subsequently reviewed in detail by the Behavioral Research Council. After the manuscript was thus processed, the various sections of it were sent to leading scientists in all fields covered. These individuals, who are listed separately as consultants, were given research grants to cover their time-consuming work involved in reviewing the various sections. In the preparation of the revised edition, additional consultants criticized the chapters as they appeared in the first edition.

The following individuals served as consultants for either the first or the revised edition: G. Lester Anderson, Yehoshua Bar-Hillel, J. Leonard Bates, Samuel H. Beer, Frederick K. Beutel, Robert Bierstedt, Hadley Cantril, John B. Carroll, Joseph B. Casagrande, Alphonse Chapanis, F. Stuart Chapin, C. West Churchman, Jere W. Clark, Henry V. Cobb, William K. Estes, Robert E. L. Faris, Ralph W. Gerard, John P. Gillin, Bertram Gross, George C. Homans, John Kirk, Samuel Krislov, Bert James Loewenberg, David G. Mandelbaum, Robert D. Meade, Delbert C. Miller, Richard K. Morris, George P. Murdock, George H. Nadel, Martin Neumeyer, Henry Ladd Smith, Walter E. Spahr, George A. Theodorson, Richard T. Vann, and John E. Weinrich.

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BEHAVIORAL RESEARCH COUNCIL

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INTRODUCTION

MODERN SCIENTIFIC INQUIRY

BY the mid-Twentieth Century the inquiries of men into problem situations had achieved enough success to encourage hope for rapid progress. The new developments are perhaps the most important "breakthrough" in the long history of man's evolutionary progression. When the significant features are generally understood and applied, inquirers into the problems of men in society may become as successful in developing warranted assertions as the physical scientists and the physiological scientists have been in recent centuries.

A highly important aspect of the new developments in inquiry is reorientation to a different goal. Apparently, from the beginning of men's inquiries into problem situations, the accepted objective was TRUTH or absolute certainty. By the time written records were kept, that goal was unquestioned. Moreover, the quest for certainty was the accepted goal even of the relatively modern scientists during recent centuries. The different goal now accepted by those who have pioneered in the latest advances in inquiry is simply a description of what happens under specified circumstances.

Ascertaining what happens is part of the scientific inquirer's job, but his task is not completed until he has provided a scientifically useful description of his findings. "Scientifically useful" designates a description that can be used by others as well as the inquirer concerned for rechecking the inquiry, or for further inquiry, or for modifying either external events or internal adjustive behavior.

The objective of scientific inquiry here suggested does not include achievement of "knowledge" in any absolute or final form. The reports of scientific inquiry are invariably provisional, always subject to revision if and when better means of observation and measurement or other improvements in procedures of inquiry make possible more useful descriptions of what happens under specified circumstances.

A second important characteristic of the suggested procedures of inquiry is that they are self-corrective; that is, included among them are the procedures for correcting them. Men have used various methods of inquiry including those of common sense, of revealed religion, of secular revelations, of seeking the aid of spooks and fairies, of consulting the oracles, of Aristotelian logic, of the philosophers' quest for certainty, and of Newtonian mechanics, to name a few. By one or more of these or other means men have claimed to find certainty, and have earnestly offered verbiage to embalm their findings in a copper-riveted, indestructible, and forever established form, never to require amendment, updating, or reconsideration.

Some men have been so sure that the methods of inquiry satisfactory to them had yielded absolute certainty that they have tied skeptics among their fellow men to stakes and burned them; and, although these are glaring examples, they may well be among the less harmful actions, all things considered, that men have done while laboring under the delusion that the methods of inquiry satisfactory to them have yielded ultimate truth.

The procedures of inquiry suggested herein are radically different and apparently are the only ones that provide for continuing development including revision as may be advisable for themselves as methods as well as for the findings of inquiry.

Traditional Inquiry

The record of man's inquiries into problem situations extends back only a few thousand years. Of course, such inquiries had been developing for uncounted centuries since the beginning of man's existence; but the details of earlier inquiries we can only infer from the artifacts that have been discovered. By the time written records were consistently kept in a form available for future generations, the early Greek philosophers had developed their ideas as to the proper objective of inquiry.

To those early philosophers, the situation seemed clear. The natural things they encountered were always changing, sometimes slowly, sometimes rapidly. Obviously, the early philosophers concluded, knowledge of what always is in the process of becoming something different cannot be eternal, absolute, and certain. Real knowledge, they assumed, must be about Being, an imagined realm, eternal and unchanging, that men can seek to know with absolute certainty.

In short, the early Greek philosophers accepted as the proper objective of serious inquiry what Dewey some two thousand years later referred to as the "quest for certainty." That certainty was attainable, that absolute TRUTH could be found, seemed to be assured by the then new development of mathematics with its apparently error-free modes of proceeding from the supposedly known to the formerly unknown. What followed has been well described by Dr. Joseph Ratner:*

> "Now the Greek theory of eternal and immutable Being and its antiphonal spectator theory of mind entered into the bloodstream of modern thought at its very inception. They entered not only by way of philosophy and religion, in which fields they had luxuriantly flourished under the fervid care of medieval logicians and theologians; more importantly, they entered by way of science—more importantly because totally unsuspected and unacknowledged there.
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> "The founders of modern science made a great show of being pure and uncontaminated philosophically. With one accord they attacked philosophy which meant, for them, medievalized Aristotelian logic and its stifling progeny.... In their march against the powers of darkness they were guided by the lamp of Euclid which

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*"Introduction to John Dewey's Philosophy," in Joseph Ratner, ed., *Intelligence in the Modern World*, New York, Modern Library, 1939. Ratner's Introduction is reprinted in Rollo Handy and E. C. Harwood, *Useful Procedures of Inquiry*, Great Barrington, Behavioral Research Council, 1973; the quotations are from pages 28, 29, 35, 40, 41, 42, and 71 of that printing.

they held aloft. But, alas, Euclid's mathematics and Aristotle's logic both involve the same basic presuppositions; they both rest on the same fundamental conceptions of knowledge, nature, and mind; they are both results of the same type of metaphysical thinking and scientific method.

* * * * * *

"The fathers of modern science, in addition to reclaiming mathematics and gaining unrestricted rights to observe nature. . .also introduced, it is true, a new and non-Greek method of experimentation. This methodological novelty was destined to become all-important in the progress of scientific knowledge and the development of scientific ideas but, at the time of its introduction, it had only a supplementary intellectual value. . . . Even in Newton's work, the most self-consciously 'experimental' of all, the supreme right-of-way was given to mathematics and not to experimental findings whenever the two came in conflict and blocked each other's path. In other words, the supreme right-of-way in the foundations of modern science was given to Greek ideas of method and science.

* * * * * *

"What is the common, ultimate destination of modern science and philosophy? . . .to say it is the eternal and immutable Reality leads us straight home, into the theoretical heart of modern science and philosophy—and back to the bosom of the Greeks.

"The first success of science in its quest for certainty was wonderfully great, so wonderful that nearly three hundred years elapsed before science matured sufficiently to have serious doubts of its own as to whether or not it had exclusively and permanently captured eternal and immutable Reality first crack out of the box. It must, however, be said immediately on behalf of most scientists today that they have not allowed themselves to become too discouraged by these doubts. Recent revelations have set them back somewhat and shaken their early confidence, but most of them still hope to succeed in finally and exclusively cornering Reality in the next try, or in the try after that, and they steadfastly aim that way.

". . .Science discovered the eternal and immutable Reality in material masses and motion and the laws governing masses in motion. It was not, of course, just the fact that they were material masses and physical motion that assured scientists they had found what they were looking for. It was the eternal, indestructible nature of the constituent particles of the masses, and the eternal, unchangeable nature of the laws of motion that proved to them, with the inerrant simplicity and unshakable certainty of mathematics, that their conviction of success was true and not the fanciful product of their dream.

"Now philosophers, like common men, have eyes and the eyes see colors—but colors, said the new science, are not ultimately real; philosophers have ears and ears hear sounds—but sounds, said the new science, are not ultimately real; philosophers have noses and noses smell smells—but smells, said the new science, are not ultimately real; philosophers have hands and hands feel surfaces, temperatures, and textures, rough and smooth, hot and cold, wet and dry, soft and hard—but soft and hot, wet and dry, cold and smooth and rough, said the new science, are not ultimately real; . . . . The only genuinely, ultimately real things are the atoms and their qualities of shape, size, hardness, motion, number, mass, inertia.*

* * * * * *

"The Greek doctrine that scientific knowledge is knowledge of eternal and immutable Reality consistently functioned to make it inconceivable for the Greeks that the world of change could be scientifically studied and known. They wrote out their own prescription for science, and their scientific activities were conducted in accordance with the directions they themselves prescribed. Consequently, though their science was restricted in fundamental character, and by our standards was hardly science at all,† they did not get into the muddle of contradictions, confusions and absurdities which has mired modern thought.

"Modern scientists, however, began by taking precisely the world of change as their subject for scientific study, and to help them on their way, they introduced the method of experimentation which is no less and no other than a method whereby the natural changes going on can be further increased and complicated in manifold ways by changes deliberately made. From the Greek point of view (and in this case, *not* excepting any Greek), this is confounding confusion, science gone insane. But as events have fully demonstrated, it is science really come to its senses, and intelligence come into its own.

* * * * * *

" 'The work of Galileo was not a development, but a revolution.'** Like all revolutions it *started* something which led to further developments. Hence it is true to say, as does Russell, that Galileo's few facts sufficed *to destroy* the whole vast system of Aristotelian knowledge. . . the new method of scientific inquiry has succeeded in finally destroying Aristotelianism in the technical fields of the most important natural sciences; but Aristotelianism (including the Platonism it both supports and is supported by) is very much alive and kicking in our current culture generally and in our social 'sciences,' philosophies and logics in particular.

"Why is it that in the technical fields of science, the revolution in *method* initiated by Galileo has already been substantially completed, has, in our time, carried through its last fundamental reform, whereas in other fields. . .the revolution is just about now seriously getting under way? The easy answer is to invoke a distinction between 'natural' sciences and 'social' sciences. . . . The 'distinction' simply repeats, as an explanation, the fact to be explained. It is the 'logic of explanation' of ancient and medieval vintage working over their time: opium puts to sleep *because* of its dormative power; . . . .

"The backwardness of philosophy, logic and all social inquiries does not explain the *forwardness* of the natural sciences. It simply exposes and emphasizes the need for an explanation. . . .

* * * * * *

"When a 'distinction' in subject-matter between the 'natural' (physical) and 'social' (mental) is used as *ground* for explaining the differences between the 'natural' (physical) and 'social' (mental) sciences, the 'distinction,' if it does not start out as a variant term for 'separation,' is forced to grow into an assertion of an abysmal separation in order to maintain itself. And when the so-called natural sciences are separated from the social, are taken out of their context in human history, and out of relation to human activity, then an adequate and satisfactory explanation of the natural sciences themselves becomes impossible.

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*The list of 'primary' or ultimately real qualities varies from scientist to scientist. The list above is a Galilean-Newtonian mixture.

†Einstein and Whitehead for instance, agree with Dewey (independently, of course) that Greek science was hardly science as we understand it now. Einstein (as far as I know) is more sweeping than Whitehead, who at least excepts Aristotle (the biologist) and Archimedes and an unnamed number of astronomers from his statement that the work of the Greeks 'was excellent; it was genius. . .But it was not science as we understand it.' *(Science and the Modern World*, p. 10).

**John Dewey, *The Quest for Certainty*, p. 94.

* * * * *

"That *corrections*, to be *scientific* (or what is the same thing, to be worthy of intelligent acceptance), must be made by methods developed by inquiry, and in response to needs of *test* growing out of inquiry, is also best seen when science is placed in the social context and when contrasted, for instance, with the 'method of correcting' science initiated and enforced by *political* demands.[*] 'Nazi science' isn't something new; it is the revival of something alas, very old. It is as old as religion. . . .The Church also coerced scientists into keeping quiet, and sometimes even succeeded in getting them to recant, witness, for example, the case of Galileo. But no scientist (or any person of intelligence) accepts Galileo's *recantation* as science. *That's* the difference.

* * * * *

" 'As long as man was unable by means of the arts of practice to direct the course of events, it was natural for him to seek an emotional substitute; in the absence of actual certainty in the midst of a precarious and hazardous world, men cultivated all sorts of things that would give them the *feeling* of certainty. And it is possible that, when not carried to an illusory point, the cultivation of the feeling gave man courage and confidence and enabled him to carry the burdens of life more successfully. But one could hardly seriously contend that this fact, if it be such, is one upon which to found a reasoned philosophy.'[†]

"Philosophic reasonings, like all reasonings, generate feelings of certainty. And no individual philosopher can ever escape from having these feelings engendered in him. A philosopher is at least as human as a scientist and usually he is more so. . . ."

* * * * *

For the purposes of this summary, enough has been said about the "quest for certainty." That "truth" or "reality" or "ultimate certainty" still is the objective of much purportedly scientific inquiry is clear. Even Einstein, much of whose life work consisted of dethroning Newton's ultimate and irrefutable "certainties," in the later years of his life devoted much time and effort to finding an ultimate "truth" more "real" and "certain" then anything he had discovered. Apparently, his emotional attachment to the long-accepted goal of inquiry had strengthened habits of thinking not readily changed.

The Objective of Scientific Inquiry

Now if an inquirer into problem situations is to discard the "quest for certainty" what is he to substitute for that objective? Progress in inquiry perhaps is imaginable without a goal in view, but in the absence of an objective how would one know whether or not progress had been achieved, and how would one know when a particular research task had been completed?

A suggestion is that the objective or goal of scientific inquiry is a description of what happens under specified circumstances. Ascertaining what happens is part of the scientific inquirer's job, but his task is not completed until he has provided a scientifically useful description of his findings. "Scientifically useful" as here applied is a name or short-hand designation for a description that can be used by others as well as the inquirer concerned for rechecking the inquiry, or as a basis for further inquiry, or as a means of modifying either external events or internal adjustive behavior, or for any combination of such purposes.

The objective of scientific inquiry here suggested does not include achievement of "knowledge" in any absolute or final form, does not purport to establish "certainty," and does not offer its findings as unalterable indestructible Truth (whatever that may be). The goal is assertions warranted by the procedures of inquiry but not alleged to be fixed and immutable. The reports of scientific inquiry are invariably provisional, always subject to revision if and when better means of observation and measurement or other improvements in procedures of inquiry make possible more useful descriptions of what happens under specified circumstances.

Useful Procedures

We begin our description of useful procedures of inquiry by noting the vast universe of the world, sun, stars, and all that we can see, smell, taste, hear, and feel. We wish to discuss the sum total of such things without repeatedly having to describe them in detail. For that purpose we need a short name, and we select "cosmos." This name is applied to the universe as a whole system including the speaking-naming thing who uses the name.

Next we differentiate (or note differences) among the vast number of things in the cosmos and select for naming the living things; for these we choose the name "organism." Note that selecting for naming does not imply detaching the physical thing from the cosmos. Everything named remains a part of cosmos with innumerable relations to other parts.

Among the organisms, we further differentiate and select for naming ourselves, our ancestors, and our progeny; these we name "man."

We then observe the transactions of man with the remainder of cosmos and note the transactions named "eating," "breathing," etc. Among the numerous transactions, we differentiate further and select for naming those transactions typical of man but rarely characteristic of other organisms.

Sign Process

Human behavior involves some transactions wherein something is regarded as standing for or referring to something else. This process we name "sign behavior," or simply "sign." Note that "sign" is not the name of the thing that is regarded as standing for something else; "sign" is the name of the transaction as a whole (i.e. the short name for "sign process"). Sign or sign process is the type of organism-environmental transaction that distinguishes a behavioral from a physiological process, from a transaction such as eating, digesting, seeing, etc.

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[*] Heisenberg, in a lecture delivered in 1934 in Berlin, said that Michelson's experiments and Einstein's theory of relativity 'belonged to the absolutely certain bases of physics.' A Dr. Rosskothen (a high school teacher) heard the lecture and wrote in complaint to Reich Director Dr. Alfred Rosenberg, Commissioner appointed by the Führer to supervise the Philosophical Instruction of the National Socialist (Nazi) Movement: 'should such a man [Heisenberg] occupy a chair at a German university? In my opinion, he should be given the opportunity to make a thorough study of the theories of the Jews of the Einstein and Michelson type, and no doubt a concentration camp would be an appropriate spot. Also a charge of treason against people and race would not be out of place.'. . . .

[†] Dewey, *The Quest for Certainty*, p. 33; italics in original.

Sign process has evolved through the following still-existing stages:

a. The signaling or perceptive-manipulative stage of sign in transactions such as beckoning, whistling, etc.

b. The naming stage used generally in speaking and writing.

c. The symboling stage as used in mathematics.

Border regions remain to be explored and characterized (i.e., tentatively named).

Focusing our attention now on the naming stage of sign process, we choose to name it "designating." Designating always is behavior, an organism-environmental transaction typical primarily of man in cosmos. Designating includes:

1. The earliest stage of designating or naming in the evolutionary scale, which we shall name "cueing." Cueing, as primitive naming, is so close to the situation of its origin that at times it is not readily differentiated from signaling. Face-to-face perceptive situations are characteristic of cueing. It may include cry, expletive, or other single-word sentences; and in fully developed language it may appear as an interjection, exclamation, abbreviated utterance, or other casually practical communicative convenience.

2. A more advanced type of designating or naming in the evolutionary scale, which we shall name "characterizing." This name is applied to the everyday use of words, usage reasonably adequate for many practical purposes of life.

3. The, at present, farthest advanced type of designating, which we shall name "specifying." This name applies to the highly developed naming behavior found in modern scientific inquiry.

For the purpose of economizing words in discourse, we need a general name for the aspects and phases of cosmos differentiated and named. For this general name we choose "fact." Fact is the name for aspects and phases of cosmos in the course of being differentiated and named by man (he himself being among its aspects) in descriptions sufficiently developed to include definite time and space aspects. Fact includes all namings-named durationally and extensionally spread; it is not limited to what is differentiated and named by any one man at any moment or in his lifetime.

Frequently, we have need to discuss a limited range of fact where our attention is focused for the time being. For this we choose the name "situation." This is the blanket name for those facts localized in time and space for our immediate attention.

Within a situation we frequently have occasion to refer to durational changes among facts. For these we choose the name "events."

Finally, in discussing events we frequently have occasion to refer to aspects of the situation involved that are least vague or more firmly determined and more accurately specified. For those we choose the name "object." Object is differentiated from event in that it is subject matter of inquiry that is relatively stable, at least for the time being.

All the subject matters of scientific inquiry are aspects and phases of cosmos, all are natural in that modern scientific inquirers do not purport to provide warranted assertions (useful descriptions) about the allegedly supernatural. Nor do modern scientific inquirers assert that nothing ever will be found beyond the scope of present means of observing by sight, smell, feeling, hearing, taste, and such extensions of sense perception as telescopes, microscopes, and other instruments at present provide.

Various subject matters of inquiry may be classified into groups from time to time in accordance with the various techniques of inquiry that may be applicable. Major classifications now widely recognized are the physical, the physiological, and the behavioral. None of these fields of inquiry is subject to the domination of one over another, yet in each an inquirer may make use of some findings in another, and all remain in the general system of cosmos becoming known by means of man's knowing behavior.

Within much of the realm of knowing behavior wherever sign process is involved, knowing is naming. Naming is application of verbal or other signs to things differentiated in cosmos. Things are differentiated by observing, hearing, touching, or otherwise noticing that this differs from that in some aspect or phase. Differences are ascertained by comparison, one thing with another, one aspect with another, etc.

Durationally and extensionally observable events are sufficient for inquiry. Nothing more real than the observable is established by using the word "real" or by attempting to peer behind or beyond the observable for something to which its name can be applied. Abandoned is the notion that "reals" exist as matter, or that "minds" exist as manifestations of organically specialized "reals," or that the "certainty" of matter somehow survives all the "uncertainties" of increasing knowledge about it. Finally, nothing is accepted or assumed in modern scientific inquiry that is alleged to be inherently nonobservable or as requiring some type of supernatural observation.

One of the most significant characteristics of the suggested methods of inquiry is that they include procedures for correcting and revising both the findings of inquiry and the methods used. The inquirer begins with what seem to be the pertinent facts in a problem situation, and he attempts to develop a description of what is happening that is adequate for solving the problem. When advance toward a solution is blocked, conjectures are imagined by the inquirer about possible connections among the facts. The conjectures that emerge from observation of facts are merged with observation of new facts (including improved description of the earlier facts). When advance again is blocked, new or improved conjectures emerge that in turn are merged with further observations; the entire process may be repeated many times in succession. In the course of inquiry, the initial problem situation may be reformulated, what seemed to be hard facts may turn out to be mistaken, facts that apparently were pertinent may become irrelevant and vice-versa, and the initially most plausible conjectures may have to be abandoned. The outcome, when inquiry is successful, is a warranted assertion; i.e., a useful description of what happens under specified circumstances. But even the best available warranted assertion is not "certain" or an embodiment of "truth"; later inquiry may lead to its modification or abandonment.

SUMMARY DESCRIPTION OF PROCEDURES FOUND USEFUL

A summary description of the procedures of scientific inquiry that we find have been useful in many instances may be helpful to readers. A primary purpose of this discussion is to comment on the technical names "conjecture," "hypothesis," and "theory." The latter two terms in particular are used in so many different ways in the current literature on scientific method that clarification is a first essential to useful discussion.

1. The objective of scientific inquiry is to provide useful descriptions of what happens under specified circumstances.

2. The adjective "useful" designates those descriptions that are adequate for such purposes as confirmation of the conclusions by duplicating the procedures insofar as may be practicable, or those descriptions of what happens that can be used to predict what probably will happen, or those descriptions of what happens that facilitate modification of future developments either by changes effected external to the human beings involved or by adjustive behavior on their part, or by various combinations of these uses.

3. An inquirer proceeds toward his goal of achieving useful description in a series of steps that may be described as follows:

a. First is awareness of a problem situation, of happenings for which a useful description is not available or is somehow inadequate.

b. Next is the attempt to ascertain the facts in the case, but the facts in the case, including the relationships among various facts, usually are not at first readily apparent.

c. Difficulties arise; i.e., development of adequate description of what is happening or has happened is blocked in one way or another, perhaps by the lack of sufficiently delicate instruments with which to observe and measure, perhaps by ignorance about some facts or aspects of facts pertinent to the investigation, or for any number of reasons.

d. Although temporarily blocked in the further development of adequate description, the inquirer does not abandon his inquiry. In imagination he develops notions or conjectures about the possible facts or relationships among facts that may be involved. Usually, many different conjectures can be developed, and the inquirer's immediate problem then is selection among them in order that inquiry may proceed. Aspects of the various conjectures suggest or point toward facts possibly not previously recognized as pertinent or in any event not yet adequately investigated. The inquirer proceeds to test one conjecture after another by returning to the facts in the case, perhaps discarding some facts at first thought pertinent, perhaps ascertaining new facts by experimental procedures or by further investigation in other ways. Eventually, adequately developed description is achieved so that the inquirer proceeds to the next point in his inquiry where again his progress temporarily is blocked.

e. Again the inquirer imagines what may have happened and new conjectures are formulated (possibly with the help of mathematical formulas) until a return to the again possibly revised facts or newly ascertained facts is possible in order once more to select the apparently most useful among alternative conjectures in order to proceed with the inquiry.

f. Ultimately, if the inquirer is successful, useful description of what happens (or what happened) under specified circumstances is achieved.

The foregoing is not an attempt to prescribe how scientific inquiry should be conducted. As we said earlier, a primary purpose is to clarify our application of "conjecture," "hypothesis," and "theory."

When we use "hypothesis" in this book, we apply it as a name for a conjecture developed in the course of inquiry, and we avoid using it otherwise except when quoting from, or discussing, the work of others.

We apply "theory" as a name for the useful description of what happens under specified circumstances that is the goal of scientific inquiry, and we shall endeavor to avoid using it otherwise except when quoting from, or discussing, the work of others (e.g., as in "Freudian theory" and "game theory").

For further discussion see the companion volume, *Useful Procedures of Inquiry*, and also pp. 7-14 of the present volume.

I.

PROCEDURES OF INQUIRY INTO HUMAN BEHAVIOR*

A. PRELIMINARY COMMENTS

IN this chapter some of the procedures of inquiry that are currently used or advocated are described; the most useful procedures of inquiry with which we are familiar are discussed in detail;* and several historical examples are considered.

"Science," "scientific method," and related names are applied in such different and inconsistent ways that their usefulness in communication frequently is limited. Agreement about the name of a particular field of inquiry (e.g., Economics) may indicate little more than rough agreement about the subject matter of the field; the most diverse procedures of inquiry into that subject matter may be used or advocated by different workers.

Economists, for example, have recommended procedures ranging from a reliance on introspection, reason unaided by experience, and the alleged structure of the human mind, to the use of the procedures so successfully applied in the physical sciences.

American psychologists for some time were moving in the direction of modern scientific inquiry, but in recent years so-called "empirical psychology" has been under attack from within the profession, not only by those advocating existentialist and phenomenological procedures, but also by those urging a revival of mentalism.

In Linguistics, which had developed rapidly as a scientific field, much attention in recent years has been given to Noam Chomsky's views. Chomsky deliberately uses mentalistic procedures, claims that the mind possesses innate knowledge, and assumes a fundamental difference between man and other animals reminiscent of traditional dualistic philosophies.

In other fields also considerable agreement about the general subject matter of the field is found, but great disagreement about the most useful procedures of inquiry. Even when there is agreement on the subject matter, the use of a particular method may preclude inquiry into some important problems, and some questions that are discussed in great detail may arise more from the assumptions of the particular method used than from what is found in the field of inquiry (e.g., questions about the interaction of mind and body, taken as basically different levels or types of reality). In addition, sometimes there is controversy about what the subject matter of a given field is. And even among those advocating scientific procedures of inquiry, there are widely varying notions of what the appropriate procedures of inquiry are.

A complex set of controversies is found, then, including disagreements about what "science" and "scientific method" designate, the extent to which various fields of inquiry can be usefully investigated scientifically, and associated matters. Two quotations will help to illustrate these controversies. R.B. MacLeod, a psychologist, says:

> "I am. . .insisting that what, in the old, prescientific days, we used to call 'consciousness' still can and should be studied. Whether or not this kind of study may be called a science depends on our definition of the term. To be a scientist, in my opinion, is to have boundless curiosity tempered by discipline." (In T.W. Wann, ed., *Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology*, Chicago, University of Chicago Press, 1964, p. 71.)

Here we see a revival of what seemed to have been eliminated from psychological inquiry and a reversion to an older use of "science" that does not emphasize observational confirmation.

Benjamin E. Lippincott, a political scientist says:

> "Empiricism, we suggest, is based upon three false conceptions. It is based. . .upon a false theory of knowledge; namely, that only 'facts' are real. The truth is that universals are as real as facts, and facts can only have meaning in connection with universals. Empiricism. . .is based on a false conception of scientific method. It tends to assume that facts somehow arrange themselves, although it is the social scientist who imparts order and meaning to them by bringing ideas to bear from within his own head. . . . Empiricism. . .assumes, with respect to social science, a false relation between the observer and his data; the observer can never be really objective with respect to his data, for he is part of it. His data are not like atoms, which are inanimate, but are human beings, like himself, which have feelings and emotions. . . .
>
> "If the foregoing analysis is correct, political theory cannot hope to develop unless it gives up empiricism, with its emphasis on description, and adopts a more creative scientific method. This does not mean throwing description overboard,. . .it means. . .that description must be more selective, and possess more meaning. We must see more clearly the part played by values and by reason (the power to draw logical inferences, to make deductions, to connect facts with propositions, and to connect one proposition with another)." ("Political Theory in the United States," in *Contemporary Political Science*, Paris, UNESCO, 1950, p. 219, p. 221.)

Here we see strong influences of traditional epistemology and philosophical rationalism, and the attribution to "empirical" scientists of views that are not held by many such inquirers.

In the past many nonscientists, especially literary people, argued against scientific inquiry into human behavior on the grounds that such inquiry made humans subservient to technology, led to the destruction of beauty, the standardizing of life, the despiritualization of man, etc. (See, for example, Morris Goran, "The Literati Revolt Against Science," *Philosophy of Science*, Vol. 7, 1940.) Those arguments are now being revived, and in the last few years some, usually younger, individuals educated as behavioral scientists have made similar arguments against the behavioral science "establishment." The recent attack on "objectivity" often is expressed in political

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*For a more detailed discussion, see the companion volume to the present book: Rollo Handy and E.C. Harwood, *Useful Procedures of Inquiry*, Great Barrington, Behavioral Research Council, 1973.

terms, and the critics sometimes maintain that many alleged behavioral science findings, far from being warranted assertions, simply express the conclusions desired by a government agency or some vested interest. Such critics frequently oppose the "ethical neutrality" of science; insist that responsible scholars must be "committed" on public issues; and argue that professional organizations such as the disciplinary associations in the scholarly fields should take official stands on the controversies of the day. The flavor of such criticisms is illustrated in a book edited by Theodore Roszak, *The Dissenting Academy* (New York, Pantheon Books, 1968), and in his *The Making of a Counter Culture* (Garden City, Doubleday, 1969).

In Roszak's latest book *(Where the Wasteland Ends: Politics and Transcendence in Postindustrial Society*, New York, Doubleday, 1972), the humanistic protests of several decades ago are again emphasized. Mysticism is advocated, scientific inquiry is said to underlie the "mad, bad ontology of our culture," a dogmatic adherence to scientific procedures is claimed to have stripped our world of purpose, spirit, and meaning, etc.

Assessing the claims and counter-claims involved in the controversies concerning the applicability of scientific procedures of inquiry is complicated because there is so much disagreement, incoherence, and confusion in the use of key terms in those controversies. In the next section we analyze some of the conflicting procedures of inquiry; in later sections we describe what appears to be the most useful type of inquiry.

B. SOME OUTMODED PROCEDURES OF INQUIRY

In our civilization many alleged "ways of knowing" have been tried and at least traces of those ways are frequently found in contemporary discussions. The purported successful outcomes of knowing also vary considerably. Many useful ways of grouping and organizing the numerous methods that have been suggested or used probably could be found. For present purposes, discussion may be facilitated by focusing attention on three general procedures (Mentalistic-Rationalistic, Formal Model Building, and Subjectivism), as contrasted with a more useful procedure discussed later. Various aspects of the methods here differentiated may be combined, and variations of the methods also can be found. Our classifying is only for convenience; we have not attempted to "carve Nature at her joints."

Mentalistic-Rationalistic Procedures. This type of procedure postulates a basic split between the mental and nonmental, which are viewed as fundamentally different and irreconcilable types or levels of reality. Knowledge about nonmental reality is said to be possible for the mind, and attainable by means of propositions or some other intervening entities that purportedly represent nonmental reality to or for the mind. Knowledge about much human behavior is alleged to be attainable through direct inspection of the mind by the mind, introspection, or some other type of action of the mind upon its own processes. So-called reasoning, of the type urged by the philosophic Rationalists, is stressed; important knowledge is believed to be attainable without recourse to observation or experience.

Illustrations can be found in many areas of inquiry. Albert Einstein once wrote: "Nature is the realization of the simplest conceivable mathematical ideas. . . . In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed." (Quoted in Gerald Holton, "Mach, Einstein, and the Search for Reality," *Daedalus*, Spring, 1968, p. 650. For a recent defense of "scientific rationalism," see Giorgio de Santillana, *Reflections on Men and Ideas*, Cambridge, M.I.T. Press, 1968.)

Noam Chomsky has not only revived a Cartesian dualism and suggested that the mind possesses innate knowledge, but believes he has found the solution to the classical problem of such a dualism. Descartes and others struggled with the problem of how two ontologically different substances—which by definition can each act only within its own realm—can interact, as when a human decides to raise his arm and does so. Chomsky believes he has found a solution in man's creative use of language, which he sees as the most important distinction between man and other animals. *(Language and Mind*, New York, Harcourt, Brace & World, 1968; *Cartesian Linguistics*, New York, Harper & Row, 1966; and "Recent Contributions to the Theory of Innate Ideas," in Robert Cohen and Marx Wartofsky, eds., *Boston Studies in the Philosophy of Science*, Vol. III, New York, Humanities Press, 1967.)

In economics, F.A. Hayek argued that human behavior must be understood from "within" and that we must interpret other people's behavior in "the light of our own mind." Ludwig von Mises argued that to understand human behavior "there is but one scheme of interpretation and analysis available, namely, that provided by the cognition and analysis of our own purposeful behavior," and also that the "ultimate yardstick of an economic theorem's correctness or incorrectness is solely reason unaided by experience." (Hayek, *The Counter-Revolution of Science*, Glencoe, Free Press, 1952, pp. 44-45, p. 77; von Mises, *Human Action: A Treatise on Economics*, New Haven, Yale University Press, 1949, p. 26, p. 858.)

A marked feature of such views is what John Dewey called the *quest for certainty.* Whatever may be found in a changing world, the mind is alleged to be so structured that it apprehends truths with complete accuracy, thereby yielding some fully assured knowledge of human behavior. This attempt to make the results of ratiocination irrefutable in principle by "facts," experience, or observation flies in the face of hard-won lessons in human intellectual history.

Probably the number of investigators of human behavior who espouse rationalistic mentalism as boldly as the writers quoted here is not large, but milder or more disguised similar views (and therefore at least potentially more dangerous) are numerous. The belief that the mind can tell us something important about human behavior in advance or independently of observation is widespread.

The "outcome" cherished by those following such procedures apparently is some sort of intellectual satisfaction. That satisfaction results from an emphasis on internal consistency, general plausibility, and a kind of secular revelation that provides "starting points," "insights," or conclusions. Such investigators apparently are reassured by a conviction that no possible observation or experience could cast doubt on the results. A "pure knowledge," then, seems to be what is searched for; the lure of which is so appealing to some that they ignore the numerous failures attributable to such procedures and the successes of inquirers who have been developing a more useful method. The sense of certainty can be so strong that important evidence is ignored. For example, although Lalande had considerable evidence for Neptune's existence, in C.D. Darlington's phrase, "He knew so well that there was no planet there that he could not discover it." *(The Conflict of Science and Society*, London, Watts, 1948, p. 4.) Many suggested descriptions of events have considerable plausibility and may accord with strongly held beliefs, but still be mistaken. In discussing the failure of some writers to note the difference between confirmation and initial plausibility, the psychologists George Mandler and William Kessen say:

> ". . .they may even provide a prodigality of plausible initial statements in their building of convincing portraits, but they and the nonempirical psychologists in general, apparently never feel the sharpest goad of the research psychologist–to find out by looking whether or not he is right." *(The Language of Psychology*, New York, Wiley, 1959, p. 250.)

Formal Model Building. Despite the criticisms made later on in this section, we are not arguing in general against the use of models–formal, mathematical, physical, or otherwise. Rather, we are pointing out misleading or uncritical uses of those models. In many respects the dangers are similar to those discussed in the preceding section, but rather than relying on the "structure of the mind" to arrive at conclusions about human behavior, sometimes workers confuse a warranted assertion about a particular model with a warranted assertion about some aspect of human behavior.

For present purposes we shall emphasize examples from game theory, utility theory, and decision theory; similar examples could be taken from information theory, general systems theory, cybernetics, and other areas. The underlying aim apparently is to develop a model that can be controlled rigorously and studied thoroughly, in the anticipation that useful light will be shed on aspects of human behavior that have been difficult to study using other techniques. But, as we shall see, the light shed may be viewed in quite different ways by different workers, and one begins to suspect that for some workers the delights of further and further manipulations of the model take precedence over useful information about human behavior.

To illustrate, in their work on game theory John von Neumann and Oskar Morgenstern hoped to show that "the typical problems of economic behavior *become strictly identical* with the mathematical notions of suitable games of strategy." *(Theory of Games and Economic Behavior*, 3rd ed., Princeton, Princeton University Press, 1953, pp. 1-2, emphasis added.) An enormous amount of attention was given to working out game theory, and it was sometimes hailed as an outstanding scientific achievement, comparable to Newton's celestial mechanics. (Anatol Rapoport, "Critiques of Game Theory," *Behavioral Science*, Vol. 4, 1959, p. 49.) Yet many became disillusioned with game theory and doubted that the various problems it was originally hoped could be solved would be solved. (R. Duncan Luce and Howard Raiffa, *Games and Decisions*, New York, Wiley, 1957, p. 10.) Probably a main factor in the disillusionment was that many predictions of human behavior resulting from game theory were not confirmed by the evidence. But this, rather than discouraging some workers, led them to view game theory as prescriptive, not descriptive. Rapoport, for example, says:

> "I think a categorical disavowal of *descriptive* content is implicit in the entire game-theoretical approach. Game theory is definitely normative in spirit and method. Its goal is a *prescription* of how a rational player should behave in a given game situation when the preferences of this player and of all the other players are given in utility units." *(Fights, Games, and Debates*, Ann Arbor, University of Michigan Press, 1960, pp. 226-227.)

But this too may generate difficulties. According to Robert L. Davis, the use of "rational" in the usual contexts of decision theory is unfortunate, for the theories do not tell the alleged rational person what he should do, but rather state *one* way in which the expected pay-off can be maximized, even though the pay-off may be obtained more easily in other ways. ("Introduction," in R.M. Thrall, C.H. Coombs, and R.L. Davis, *Decision Processes*, New York, Wiley, 1958, pp. 4-5.) In a later paper Rapoport agrees that game theory "ceases to be normative" in some situations, and says that game theory is "neither prescriptive nor descriptive," but is a structural theory that describes in mathematical terms "the *logical* structure of a great variety of conflict situations." ("Game Theory and Human Conflict," in Elton B. McNeil, ed., *The Nature of Human Conflict*, Englewood Cliffs, Prentice-Hall, 1965, p. 196.)

In many game and decision theories the most that can be said is that if a person finds himself in a specified type of situation and if all the players behave according to the specified rules, etc., then solutions are available that will tell him a way to achieve as much as can be achieved in such situations. But that seems a far cry from the grandiose claims sometimes made for those fields.

The internal consistency, mathematical rigor, and ingenuity used in developing these models may all be of an exceptionally high order, and perhaps the satisfaction gained in doing such work is sufficient for many workers. But that alone is no warrant for saying either that the theories predict or otherwise illuminate human behavior, or that the prescriptions given are useful in solving human problems. For example, much work in game theory is on "zero sum" games in which the winnings of some players must be equal to losses by other players. In economic competition, on the other hand, there need not be winners and losers in that sense, but rather a general improvement or net gain for all involved can occur. The conditions essential to a game theory solution, then, may contradict what is found in the human behavior the theory is designed to illuminate.

Some of the main reasons models of the type we are discussing often yield poor predictions are:

(1) To develop the theory, assumptions are made that are clearly inconsistent with the type of situation in which the predicted human behavior occurs, such as "perfect knowledge" assumptions;

(2) Doubtful assumptions are made, such as that utility is substitutable and unrestrictedly transferable among the players;

(3) Difficulties occur in correlating something within the model to something outside it, such as a numerical measure in the game and a person's "utility function." (See von Neumann and Morgenstern, *op. cit.*, p. 604; Luce and Raiffa, *op. cit.*, p. 5; Rapoport, 1959, *op. cit.*, p. 65; and Ewald Burger, *Introduction to the Theory of Games*, trans. by John E. Freund, Englewood Cliffs, N.J., Prentice-Hall, 1963, p. iii. For a general discussion of some of the key issues, see Ch. VI, "Utility, Rationality, and Formal Approaches to Value," in Rollo Handy, *The Measurement of Values*, St. Louis, Warren H. Green,

1970.)

When the models are taken as descriptive, there often seems to be relatively little attention given to external testing. With reference to models, such as an information-handling model for human behavior, that have been used by many workers in psychology, Alphonse Chapanis notes that: "Even when we find model builders attempting to make some validation of their models we sometimes find them using as scientific evidence the crudest form of observations collected under completely uncontrolled conditions." ("Men, Machines, and Models," *American Psychologist*, Vol. 16, 1961, p. 130.)

Luce and Raiffa regard utility as "an indispensable tool" for their book, emphasize how difficult it is "to determine a person's utility function even under the most ideal and idealized experimental conditions," but urge more work on that topic. They then say:

> "If it is so difficult to determine utility functions under the best of conditions, there is certainly no hope at all that it can be done under field conditions for situations of practical interest. Thus, if the theories built upon utility theory really demand such measurements, they are doomed practically; if they can be useful without making such measurements, then why go to the trouble of learning how? As in the physical sciences, we would claim that a theory may very well postulate quantities which cannot be measured in general, and yet that it will be possible to derive some conclusions from them which are of use. . . .The main purpose is to see if under any conditions, however limited, the postulates of the model can be confirmed and, if not, to see how they may be modified to accord better at least with those cases. It will still be an act of faith to postulate the general existence of these new constructs, but somehow one feels less cavalier if he knows that there are two or three cases where the postulates have actually been verified." (Luce and Raiffa, *op. cit.*, p. 12, pp. 36-37.)

The remark just quoted seems to express a certain ambivalence: if one wishes not to be "cavalier," testing of the "postulates" is called for, yet the inability to test thoroughly is not taken as fatal to the existence of the postulates. If one adopts the view of some inquirers that it does not matter if the "postulates," "assumptions," etc., are "contrary to fact" or "distort reality," so long as good predictions result from the "theory," we still have the embarrassment that such predictions are not forthcoming in practice. And, if the descriptive role is given up, we have other problems about how useful the "theory" is in *prescribing* behavior.

As a general statement of what is often found in uses of formal models, we note the following stages:

(1) Various assumed notions about human behavior are taken as sound (axiomatic, or truisms, or as otherwise assured).

(2) Those notions are translated into a formal model (mathematical, cybernetic, etc.).

(3) Numerous transformations, perhaps requiring marked ingenuity and technical proficiency, are made within the model.

(4) The results of those transformations, when translated out of the model, are assumed to provide illumination of typical problems of human behavior.

As stated, one may question whether this type of quest for certainty does occur. Two examples will be given:

(a) George Homans developed a model of "elementary social behavior" based on five "postulates" reflecting some notions found in economic "theory" and in behaviorist psychology. (*Social Behavior: Its Elementary Forms*, New York, Harcourt, Brace & World, 1961.) He viewed elementary social behavior as "face-to-face contact between individuals, in which the reward each gets from the behavior of the others is relatively direct and immediate." (*Ibid.*, p. 7.) His five "postulates" follow:

> (1) "If in the past the occurrence of a particular stimulus-situation has been the occasion on which a man's activity has been rewarded, then the more similar the present stimulus-situation is to the past one, the more likely he is to emit the activity, or some similar activity, now."
>
> (2) "The more often within a given period of time a man's activity rewards the activity of another, the more often the other will emit the activity."
>
> (3) "The more valuable to a man a unit of the activity another gives him, the more often he will emit activity rewarded by the activity of the other."
>
> (4) "The more often a man has in the recent past received a rewarding activity from another, the less valuable any further unit of that activity becomes to him."
>
> (5) "The more to a man's disadvantage the rule of distributive justice fails of realization, the more likely he is to display the emotional behavior we call anger." (*Ibid.*, pp. 53, 54, 55, 75.)

The usefulness of these conjectures might be questioned in view of their reliance on marginal utility notions, and one wonders if differing cultural settings and social structures might not make an important difference in social behavior. Homans has great confidence in his postulates, despite the lack of confirming evidence. He says:

> "At the level of elementary social behavior there is neither Jew nor Gentile, Greek nor barbarian, but only man. Although I believe this to be true, I cannot demonstrate that it is so. . . .Accordingly, though I believe that the general features of elementary social behavior are shared by all mankind, I believe it as a matter of faith only, and the evidence that I shall in fact adduce is almost wholly American." (*Ibid.*, pp. 6-7.)

Our present interest is in what other workers have done with Homans' postulates. John and Jeanne Gullahorn have developed a computer model of elementary social behavior relying heavily on Homans' model. Their report shows how difficult the technical construction of such a model can be. They are enthusiastic about the uses of the computer model, but at the time of their report had done very little in the way of testing the model against observed behavior. They say: "The simulation appears to have verisimilitude, but its verity has not yet been tested against actual social interaction." (John T. and Jeanne E. Gullahorn, "A Computer Model of Elementary Social Behavior," in Edward A. Feigenbaum and Julian Feldman, eds., *Computers and Thought*, New York, McGraw-Hill, 1963, p. 376.)

Roger Maris has recently worked in detail on the internal consistency of Homans' five postulates. ("The

Logical Adequacy of Homans' Social Theory," *American Sociological Review*, Vol. 35, 1970.) Maris concludes that the postulates are probably logically consistent and that if certain assumptions are made, some of which he regards as questionable, many of Homans' other conclusions can be logically deduced from his five postulates.

Here we have a logical model and a computer model constructed from an original postulate model, both of which may provide useful information about Homans' original model, but which do not go to the heart of the matter in the sense of assessing how warranted the original model was.

(h) In 1953 Arnold Tustin published a book in which he applied a feedback analysis to, and developed an engineering model for, some of the economic notions (investment multiplier; the conclusion that investment equals savings) in J.M. Keynes' *General Theory*. (Tustin, *The Mechanism of Economic Systems*, London, Heinemann, 1953.) In commenting on Tustin's work, Stafford Beer writes enthusiastically:

> ". . .no one would claim that the model is wholly comparable with a real-life economic system. What has been achieved, surely, is insight into economic mechanisms, and an implicit promise that a scientific attack on certain limited economic problems (hitherto, perhaps, regarded as unapproachable, or unstateable, or even as manifestations of divine wrath) may prove possible. . . .With the aid of the simulation techniques of operational research, then, it must be possible to construct a model of an economy competent to handle at any rate some real-life problems. With the aid of analogue engineering it may further be possible to experiment on the model, and to use it as a guinea-pig. And if all this is possible, then the economy becomes a fit subject for cybernetic control rather than guesswork and the vapourings of political theorists." *(Cybernetics and Management*, New York, Wiley, 1959, p. 35.)

But if Keynes' notions turn out to be mistaken, all we have are models upon models of a misleading "theory," which may turn out to be equivalent to the "vapourings of political theorists."

Subjectivism. The material discussed in this section overlaps to some degree that discussed under Mentalism, but the subjectivisms discussed here do not necessarily involve the assumption of a separate mind. "Subjectivism" is a loose term and has been applied to many procedures of inquiry, some of which have a strong "intellectual" flavor and some of which are highly "emotional." At present there is considerable interest, in many behavioral science fields, in existentialist, phenomenological, and humanistic procedures, all of which focus attention on "inner" processes in human behavior. (For examples of such methodological themes, see William D. Hitt, "Two Models of Man," *American Psychologist*, Vol. 24, 1969; Abraham H. Maslow, "What Psychology Can Learn from the Existentialists," Ch. 2 of his *Toward a Psychology of Being*, 2nd ed., New York, Van Nostrand Reinhold, 1968; and Charlotte Buhler, "Basic Theoretical Concepts of Humanistic Psychology," *American Psychologist*, Vol. 26, 1971.)

For present purposes, we shall focus on two aspects of this general approach:

(1) The reliance on some inner sense of correctness, truth, or warranty, such as intuitions. This can be found in highly intellectual, technical, and quantitative settings. Robert H. Strotz, for example, in a discussion of cardinal utility, emphasizes the "strong intuitive appeal" of some game theory axioms, and maintains that every "normal person would clearly accept them as precepts of behavior." ("Cardinal Utility," *American Economic Review*, Vol. XLIII, 1953, pp. 391-393.) This typifies a classical deductive quest for certainty in which some certain or unchallengeable starting point is "found," and then later transformations of that starting point are believed to yield similarly "perfect knowledge."

The appeal to some private, inner sense is also found, not at the beginning of an inquiry, but at the end. An interesting example is found in the work of the physicist, P. W. Bridgman. His views are complicated, and he frequently exhibits remarkable candor. He says that he is "becoming more and more conscious that my life will not stand intelligent scrutiny." Although much of his methodological work concerns problems discussed extensively by philosophers, he says that his "reading of philosophical literature has been very limited." He struggles with many issues at great length and without apparent progress, and at one point notes that his readers may wonder why he is trying to make a certain point at all.

In his discussion of procedures of inquiry Bridgman strongly rejects such notions as that "thought is the measure of all things," that "conclusions can be drawn endowed with an inescapable necessity," and that "mathematics has an absolute validity and controls experience." He also rejects some forms of solipsism, but yet says that "nothing *matters* except what I am aware of," and that "direct experience embraces only the things in my consciousness." Implicit in his view is the notion of a mind possessing psychic powers; the "objectivity" of tables, stars, etc., is generated when different minds react in a similar way.

Bridgman rejects the view that "public confirmation" is a central aspect of scientific method. He argues that in the last analysis it is always one individual who finally accepts a conclusion. He notes the possibility that he might, under extreme circumstances, make the judgment that all his fellow workers had gone insane, and concludes: "The criteria are thus ultimately my own private criteria, and in this sense physics or mathematics or any other science is my private science." He also argues that the final test in scientific inquiry is when the conclusion "clicks" for the individual. *(The Nature of Physical Theory*, Princeton, Princeton University Press, 1936, pp. 13-15, pp. 135-136; *Reflections of a Physicist*, 2nd ed., New York, Philosophical Library, 1950, pp. 36-61, p. 347; *The Intelligent Individual and Society*, New York, Macmillan, 1938, p. v, p. 1, pp. 152-153, pp. 157-159.)

(2) Rather than an appeal to some inner certainty either at the beginning or the end of an inquiry, as has just been discussed, some writers argue that inquiry into human behavior must be subjective in the sense that an understanding of inner states, such as feelings, is necessary to such an inquiry, while natural science procedures of inquiry are restricted to external happenings. This general point is advocated by workers in a variety of fields.

F.A. Hayek, for example, says that there is *no* obstacle to investigating "unconscious reflexes or processes" in the human body scientifically, and as "caused by objectively observable external events." However, he goes on, the social sciences (in the sense of what formerly were called the moral sciences) are "concerned with man's conscious or reflected action" in which choice is possible. Inquiry into such behavior is based on "our subjective knowledge of the workings of the human mind" and "what we can see only from the inside." (Hayek, *op. cit.*, pp. 25-26, p. 44, p. 59.) Such views are frequently found in *Verstehen* theories. William Dray, for example, holds that human actions have to be understood from "the actor's point of view" and insists that "only by putting yourself in the agent's position can you *understand* why he did what he did." Even though many human behaviors are lawful, "discovery of the law would still not enable us to understand them in the sense proper to this special subject-matter." *(Laws and Explanation in History*, London, Oxford University Press, 1957, p. 118, p. 128.)

For a pointed criticism of views that much human behavior can be understood only from the "inside," see May Brodbeck, "On the Philosophy of the Social Sciences," *Philosophy of Science*, Vol. 21, 1954, and Richard Rudner, "Philosophy and Social Science," *Philosophy of Science*, Vol. 21, 1954. Both are reprinted in E.C. Harwood, *Reconstruction of Economics*, 3rd ed., Great Barrington, American Institute for Economic Research, 1970; a volume in which several other relevant methodological issues are also discussed.)

As a final example, Peter Winch argues that there is a basic difference between the "concepts" of the physical and the social sciences. Conceptions used in the social sciences "enter into social life itself and not merely into the observer's description of it." Winch argues that social scientists often err by regarding as empirical questions what are *a priori* conceptual questions. Physical science inquiry aims at prediction, but human decisions cannot be definitely predicted; "if they could be, we should not call them decisions." He maintains that the "central concepts which belong to our understanding of social life are incompatible with concepts central to the activity of scientific prediction." *(The Idea of a Social Science and its Relation to Philosophy*, London, Routledge & Kegan Paul, 1958, pp. 93-95.)

Without in the slightest denying that human emotions, feelings, etc., are important, one may question the usefulness of alleged explanations given in terms of a sympathetic understanding of the behavior of oneself or others, inasmuch as supporters of *Verstehen* often emphasize that they are supplying "explanations" of behavior rather than "mere descriptions." Arthur F. Bentley, at least as long ago as 1908, pointed out that alleged explanations in terms of feelings often are tautological. He gives as an illustration a person who, upon seeing a man bullying a boy, knocks the bully down. Bentley asked why the hero behaved as he did, and was told that sympathy for others motivated the action. Bentley then asks why the sympathy was expressed in some social settings but not in others:

> "The man who got the praise from the crowd is known to me. Half a mile from where he lives there are women and children working their lives out for less than a nourishing living. Nearby an old woman starved to death a few days ago. Child-labor under most evil conditions is common in the city. A friend of his is making his wife's life a burden by day and a horror at night. Yet he does not intervene to save the starving, or to alleviate the condition of the half-fed workers. He does not join the society for the prevention of child-labor. He does not use his influence with his friend to show him the brutality of his ways. . . .When my friend said that sympathy had moved the man to his act, he did, then, but restate in other words the very question I had asked." *(The Process of Government*, Bloomington, Principia Press, 1935 ed., p. 3, p. 6.)

That Bentley's criticism of motive-explanations as frequently only restatements of the behavior to be accounted for is still pertinent is illustrated by the following recent quotation from a psychologist:

> "Thus, other things equal, the person described as strong in achievement motive should be more willing to initiate achievement-oriented activities when presented with challenging opportunities in his environment and should be more persistent in them when confronted with opportunities to engage in other kinds of activity than the person described as weak in achievement motive." (John W. Atkinson, "Change of Activity: A New Focus for the Theory of Motivation," in Theodore Mischel, ed., *Human Action: Conceptual and Empirical Issues*, New York, Academic Press, 1969, p. 130.)

The procedures of inquiry described in this section all rely in some important way on an appeal to some inner knowledge. In view of the human record, one might well suggest strong skepticism toward whatever seems subjectively certain or unquestionable. Hunches, intuitions, senses of certainty, and what is self-evident have been wrong so often, and can so impede progress, that the persistence of defenses of subjective methods is surprising. Two possible clues to that persistence will be mentioned here.

First, subjectivisms (extreme or mild) tend to assume the separation in some fundamental way of man and the rest of the cosmos. Even if there is no commitment to a fully developed or clearly thought out epistemological or ontological dualism, a dualism is accepted methodologically. Once human thinking, feeling, etc., is regarded, not as biosocial adjustive behavior, but as psychic or mentalistic products or processes (contrasted in some fundamental way to the nonhuman), one faces a gap between the two realms that cannot be bridged or can only be bridged with difficulty. One is likely to generate problems about "privacy," "how the mind can know the world or other minds," etc. The long and intrenched dualistic tradition in Western civilization may make acceptance of dualism (even if one is not fully aware of the dualism) seem so normal that challenging it is nearly unthinkable.

Second, some purported scientific procedures that reject the mind, mentalism, consciousness, etc., still reflect some of the consequences of dualistic and spiritistic assumptions. Some forms of American behaviorism fall into that category. Beginning with a Cartesian mind-body dualism, many behaviorists and materialists rejected the mental half of the dualism and constructed their methodologies on what was left, but without rejecting thoroughly the entire framework of a mind-body split. (For trenchant comments, see J.R. Kantor, *The Logic of Modern Science*, Bloomington, Principia Press, 1953, pp. 258-259; and John Dewey and Arthur F. Bentley, *Knowing and the Known*, Boston, Beacon Press, 1949, paperback edition 1960, pp. 130-132.)

Either the brain (or the brain plus other parts of the body) is given many of the old psychic functions of the mind, or much that is distinctively human is overlooked. Just as, at present, physical investigative techniques alone are not adequate for inquiry into physiological subject matters, so physiological techniques alone are not adequate for much human behavior. The failure of some workers to come to grips with distinctive human behavior on the basis of physical and physiological investigative techniques does not, of course, show that subjectivist methods are useful, but may lead some to turn to subjectivism.

The issues can become complex and subtle. B.F. Skinner, for example, says:

> "An adequate science of behavior must consider events taking place *within the skin* of the organism, not as physiological mediators of behavior, but as part of behavior itself. It can deal with these events without assuming that they have any special nature or must be known in any special way. . . .Public and private events have the same kinds of physical dimensions." (In Wann, *op. cit.*, p. 84, emphasis added.)

Here Skinner rejects a bifurcation between public and private events and nonscientific inquiry into human behavior, yet his statement suggests a detaching of the organism from its environment much as the mind is separated from its body in older dualisms. A pointed critique of such localizations of behavior has been made by A.F. Bentley. ("The Human Skin: Philosophy's Last Line of Defense," *Philosophy of Science*, Vol. 8, 1941.)

Intrenched cultural traditions of inquiry can be exceptionally difficult to overcome, and traces may occur even in writers who pride themselves on having rejected spiritistic or mentalistic procedures. Viewing human behavior as organism-environment transactions, which will be discussed in detail later, requires much vigilance to avoid falling into the traps that have impeded so many past attempts to arrive at warranted assertions.

C. A MORE USEFUL PROCEDURE OF INQUIRY

General Account of Procedure. We have discussed procedures in which the outcomes for many inquirers were plausible and intellectually satisfying reports, and which sometimes reflected confidence that the results were guaranteed to hold independently of observations. The procedures presently under discussion are not expected to yield such certainty or finality. The hope, rather, is for useful results, and the outcome is the development of warranted assertions enabling us to predict events and thus in some degree to control those events or to adjust our responses to them. (Accurate predictions of adverse weather, for example, enable us to take appropriate protective action even if we cannot prevent storms.)

Rather than the development of satisfying and plausible reports, the objective is the development of increasingly accurate descriptions of what happens under specified circumstances. Many commentators argue that "explanation" should be the goal, and maintain, for example, that description tells you only that grass is green, but an explanation tells you why grass is green. But explanations, when analyzed, always seem to be descriptions of connections among things and events. Even so-called "ultimate explanations" are descriptions of presumed general invariant connections, such as that Nature, or God, or Reality, is "just that way."

Einstein, for example, once maintained that the aim of physical theory is "to help us not only to know *how* Nature is and *how* her transactions are carried through, but also to reach as far as possible the perhaps utopian and seemingly arrogant aim of knowing why Nature is *thus and not otherwise.*" (Holton, *op. cit.*, p. 659.) But one can always ask: "Why should Nature be that way; what is the ultimate explanation of that?"

In everyday life we often ask for an explanation when something untoward, surprising, inconvenient, or disconcerting happens; e.g., we ask why the car would not start. The "explanation," that the starter gear broke, points to what needs to be repaired so that the car will again function normally. Such explanations are descriptions of various things, events, and their relations.

Both descriptions and observations involve abstractions from the totality of things and events in the field of inquiry. Some observations, measurements, and descriptions, although not erroneous, may be irrelevant or trivial (if one is attempting to decrease traffic congestion in the Lincoln Tunnel, presumably the most meticulous record of the serial numbers of the automobiles passing through will not be useful). Further inquiry may lead to more and more useful observations and descriptions, all of which abstract among (focus attention on some) aspects and phases of the total situation.

The view–seemingly supported not only by common sense but by Aristotle's metaphysics–that heavier bodies by their very nature fall faster than lighter bodies, was an accepted report that accorded with some observations (for example, a feather encounters so much air resistance that it falls much slower than a lead ball). One of the early experiments by Simon Stevin involved two lead balls, one ten times heavier than the other, dropped from a height of thirty feet on a sounding board. Stevin found that the two balls reached the board so nearly at the same time that only one sound was heard. He also found the same to occur when balls of the same circumference, but differing ten-fold in weight, were used. *(De Beghinselen des Waterwichts,* 1586; Stevin's report is quoted in F. Sherwood Taylor, *A Short History of Science and Scientific Thought*, New York, Norton, 1949, p. 100.) Admittedly by later standards Stevin's measurements were crude, but they still were useful and provided important clues for more precise measurements.

The technology available to an inquirer often is a major factor in what can be observed. In Galileo's account of his experiments on balls rolling down an inclined plane, he emphasizes the care he took to minimize friction by making the balls "very hard, round and smooth," and in making the surface of the groove in the inclined plane "polished and smoothed as exactly as can be." He was not able to measure time more accurately than to 1/10 of a pulse beat, which he accomplished by an ingenious water clock. Within those technological limitations, he found that the distances the balls traveled were always proportional to the square of the times involved. *(Dialogues Concerning Two New Sciences,* 1638; Galileo's report is quoted in Taylor, *op. cit.*, pp. 100-101.)

Michael Scriven has observed that in a sense Galileo was fortunate in the instruments available to him, for they were sufficiently sensitive to reveal the "law" of falling bodies, but not sensitive enough to reveal mounting energy losses as the size of the balls, or the distance traveled, increased. ("A Possible Distinction between Traditional Scientific Disciplines and the Study of Human Behavior," in Herbert Feigl and Michael Scriven, eds., *Minnesota Studies in the Philosophy of Science*, Vol. I, Minneapolis, University of Minnesota Press, 1956, p. 335.)

This leads to another issue about which confusing things have been said. Anatol Rapoport, for example, in a criticism of views emphasizing the importance of observation, says that mathematical physics "would have never left the ground" without "ideally true and factually false laws" such as Galileo's law of falling bodies. ("Various Meanings of Theory," *American Political Science Review*, Vol. LII, 1958, p. 983.) Here again we should ask what the objective of inquiry is. If the goal is plausibility of some sort, "ideally true laws" may satisfy, but if we wish to predict and control, the "factual falseness" is a problem. A statement of possible relations, such as Galileo's law, may be useful within a certain range because our measuring devices cannot detect deviations, or because the deviations are so small they do not matter, and may be useful as approximations in other ranges because corrections (for energy losses, etc.) can be made. A statement of connections so "ideal" that it does not apply to any situations we are investigating may even be helpful if we can use the "law" together with some warranted assertions to arrive at new warranted assertions. But to praise the "contrary to fact" aspect of a generalization seems peculiar, because use in inquiry depends on our ability to compensate for the inaccuracies.

A warranted assertion, then, does not represent Truth, perfect knowledge, or certainty. The name itself suggests that attention be given to what the warrant is; i.e., the adequacy of the evidence supporting the assertion. As inquiry progresses, a warranted assertion may be changed and made more accurate; the assertion is always open to inspection, challenge, and modification in the light of new evidence (e.g., successive determinations of the speed of light). Within the limits (technological and otherwise) of Galileo's experiment, his "law" held exactly; under other circumstances the "law" does not describe accurately what is found. The latter is neither a derogation of Galileo's achievements, nor a basis for praising his findings as "ideally true."

Although perfect or complete knowledge no longer is regarded as the goal of inquiry, one merit of the type of inquiry practiced so successfully in modern scientific investigations is the high level of agreement often reached. The reliability of the best scientific findings is such as to overcome (usually) many other differences—political, ideological, cultural. This is in marked contrast to other procedures, where practitioners of the same method often arrive at vastly different results, perhaps all of which are alleged to be certain. Even when strong political pressure is used on behalf of a scientific hypothesis or conjecture[*] contradicted by the evidence (e.g., Lysenkoism), the weight of the evidence often is enough to displace eventually the politically supported conclusion.

The continuing cycle of checking conjectures or hypotheses against the evidence, developing modified conjectures on the basis of new evidence, etc., with no final stopping point envisaged, is the ongoing process of inquiry. This is, of course, not to deny that at any given time, errors can occur or that the data will seem to support what later turns out to be mistaken. W.I.B. Beveridge, for example, notes a situation that has its parallels from time to time:

> "In Dr. Monroe Eaton's laboratory in the United States influenza virus can be made to spread from one mouse to another, but in Dr. C.H. Andrewes' laboratory in England this cannot be brought about, even though the same strains of mice and virus, the same cages and an exactly similar technique are used." *(The Art of Scientific Investigation*, London Heinemann, 1951, p. 24.)

From time to time an apparently sound experimental test refutes a conjecture, but later work shows that unnoticed defects in the test accounted for faulty results and the original conjecture is confirmed. For example, Kaufmann's early experimental test of Einstein's work on relativity gave results incompatible with Einstein's conjectures, and other competing conjectures yielded predictions closer to Kaufmann's experimental findings. After several years, physicists concluded that Kaufmann's equipment was not adequate for his inquiry; what seemed to be "hard" or correct data incompatible with a theory turned out to be mistaken. (See Holton, *op. cit.*, pp. 651-652.)

And Hertz, who suggested that if cathode rays consist of particles a magnetic field would be developed, found that experimentally he could not detect the predicted magnetic field. Later he suggested that if cathode rays were discharged between negatively and positively charged plates, the cathode rays should be deflected by the charged plates. That too failed in his experimental test. However, his conjectures later were confirmed; his experiments had been defective. (See L.W. Taylor, *Physics: The Pioneer Science*, Boston, Houghton Mifflin, 1941, p. 776, and Kantor, *op. cit.*, p. 108.)

Often conjectures that were developed on the basis of general speculation, but which were untestable for a long time, or at least not tested, later are confirmed. J.R. Kantor has summarized some of those conjectures about heat:

> "A fascinating and paradoxical feature of the physics of heat is the early development of ideas which, though later abandoned, finally turned out to be exceedingly satisfactory. These are the variants of the notion that heat is, or is connected with, motion or agitation of particles. Beginning with Bacon (1620), Descartes (1637), and Hobbes (1635), the following philosophers and scientists proposed this view, though for different reasons: Amontons (1702), Boyle (1738), Cavendish (1783), Locke (1706), Newton (1692, 1717), Huygens (1690), Hooke (1678), Daniel Bernoulli (1738), Lavoisier and Laplace (1783) and Davy (1799)." (Kantor, *op. cit.*, pp. 173-174.)

This suggests the importance of continued interweaving between conjectures and data rather than seeking for permanent establishment of a scientific conclusion or a refutation for all time of a hypothesis by an experiment. The desire for an ultimate solution is often found. For example, in an article minimizing the relevance of history of science to methodology, Norwood R. Hanson attaches great importance to the point that frequently the best argument, based on the best data available at a given time, is not the argument "that is ultimately correct." He further characterizes as "an exciting disclosure" that "scientific advance and rigorous logic do not always walk arm in arm"; a "disclosure" that would hardly come as a surprise unless one were strongly influenced by the quest

[*] In the methodological literature, "hypothesis" sometimes designates any conjecture about possible connections among facts, sometimes designates only the relatively exact formulations that may occur in the later stages of inquiry, and sometimes is used in a context emphasizing logical deduction. To avoid possible confusion, we prefer to use "conjecture" as applying to any tentative notion about possible connections.

for certainty. ("The Irrelevance of History of Science to the Philosophy of Science," *Journal of Philosophy*, Vol. LIX, 1962, p. 581, pp. 578-579.)

John Dewey pointed out some thirty-five years ago that in scientific inquiry both the initially selected facts and the plausible conjectures about them probably will be modified as inquiry progresses; that scientific conclusions taken to be "finally true" normally obstruct inquiry; and that typically in the course of inquiry "correct conclusions have been progressively reached from incorrect 'premises.' " *(Logic: The Theory of Inquiry*, New York, Holt, Rinehart and Winston, 1938, pp. 142-143.)

One of the most significant characteristics of modern scientific methods of inquiry is that they include procedures for correcting and revising both the findings of inquiry and the methods used. The inquirer begins with what seem to be the pertinent facts in a problem situation, and he attempts to develop a description of what is happening that is adequate for solving the problem. When advance toward a solution is blocked, conjectures are imagined by the inquirer about possible connections among the facts. The conjectures that emerge from observation of facts are merged with observation of new facts (including improved description of the earlier facts). When advance again is blocked, new or improved conjectures emerge that in turn are merged with further observations; the entire process may be repeated many times in succession. In the course of inquiry, the initial problem situation may be reformulated, what seemed to be hard facts may turn out to be mistaken, facts that apparently were pertinent may become irrelevant and vice-versa, and the initially most plausible conjectures may have to be abandoned. The outcome, when inquiry is successful, is a warranted assertion; i.e., a useful description of what happens under specified circumstances. But even the best available warranted assertion is not "certain" or an embodiment of "truth"; later inquiry may lead to its modification or abandonment.

The matters just discussed are related to the question of the most useful beginning point in inquiry. John Dewey contrasted the "gross macroscopic, crude subject-matters in primary experience" with the "refined, derived objects of reflection." The macroscopic subject-matter "sets the problems of science and furnishes the first data" of reflection, and the results of reflection are tested by "return to things of crude or macroscopic experience—the sun, earth, plants and animals of common everyday life." *(Experience and Nature*, 2nd ed., La Salle, Open Court, 1929, Ch. I.)

Joseph Ratner concludes that "the fundamental methodological problem of all inquiry" concerns the starting point of inquiry, and that the rise of modern scientific inquiry stems from Galileo's beginning with macroscopic subject-matters rather than "the refined objects of reflection which constituted the Aristotelian-medieval 'science of physics.' " ("Introduction," in Joseph Ratner, ed., *Intelligence in the Modern World*, New York, Modern Library, 1939, p. 91.)

Other investigators begin their inquiries with refined objects of reflection. Ratner considers several instances of writers who take formal logic and mathematics as the model for scientific inquiry, and the difficulties those writers encounter. Other illustrations can be found in recent discussions. For example, the sociologist Arthur L. Stinchcombe says that it is more important for contemporary sociologists to invent theory than it is for them to test it. *(Constructing Social Theories*, New York, Harcourt, Brace and World, 1968.) Some authors who emphasize testing argue that it is permissible to start inquiry with the development of elaborate hypotheses, if they eventually are tested.

However, if conjectures are allowed to proliferate in the sense that an untested conjecture is assumed to be correct and further untested conjectures are based upon it, the chances of arriving at a useful conclusion are small. To illustrate, if there are 10 stages in an inquiry, and 10 initially plausible conjectures at each stage, the chance of an inquirer selecting the correct conjecture 10 times in succession is only 1 in 10 billion. Even if there were only 2 alternative conjectures at each of 10 stages, the chances of selecting all the correct or more useful conjectures would only be 1 in 1,024. The interweaving of conjectures and measurements of change at each stage in inquiry, then, is the procedure offering most hope for success.

The interweaving just described was emphasized many times by John Dewey. In the passage quoted below, he uses "idea" for what we call "conjecture." When inquiry is blocked, the inquirer moves beyond the immediate facts or presumed facts:

> "A flight away from what there and then exists does not of itself accomplish anything. It may take the form of day-dreaming or building castles in the air. But when the flight lands upon what for the purpose of inquiry is an idea, it at once becomes the point of departure for instigating and directing new observations serving to bring to light facts the use of which will develop further use and which thereby develop awareness of the problem to be dealt with, and consequently serve to indicate an improved mode of solution; which in turn instigates and directs new observation of existential material, and so on and on till both problem and solution take on a determinate form. . . .Every *idea* is an *escape*, but escapes are saved from being *evasions* so far as they are put to use in evoking and directing observations of further factual material." *(Knowing and the Known*, p. 319.)

Closely related to this issue are questions concerning systematization and deducibility. Some writers argue that the more advanced a scientific area is, the more the warranted assertions in that area can be deduced from a relatively few generalizations or "laws." Such views have encouraged some behavioral scientists to develop very inclusive hypothetical structures and to emphasize a deductive structure within those structures. But, as Ratner points out, "Galileo's badly systematized 'few facts' " were scientifically superior to Aristotie's well-systematized structure that purportedly "explained" so much. And Newton's physics, although more developed as a formal deductive system than the physics that superseded it, was far less useful than the later physics. (Ratner, *op. cit.*, pp. 92-93.) What facilitates prediction and control, then, need not be part of a highly developed deductive system.

Two other aspects of much scientific inquiry—quantification and experimentation—also should not be viewed uncritically. Detailed and precise quantification often is possible and useful. But at times, especially in the early stages of inquiry, crude quantification (expressed in terms such as "more," "less," "nearly," etc.) also can be useful. Stevin's early experiment with falling lead balls, for example, had no better measure of elapsed time than listening to the sound of the balls hitting a board, and yet this was sufficient to show how erroneous the accepted views were. Anyone who insists on detailed and precise quantification as being essential to scientific method may waste time and effort on unhelpful quantification or focus uncritically only on what is precisely quantifiable at a given time.

Controlled experiments in the course of which the consequences of a deliberate change in one aspect or phase can be measured also are usually useful. But useful measurements of change can be made in other circumstances (e.g., astronomy) and yield important warranted assertions. A "fixation" on experimentation may lead to a neglect of problems that could be studied by other techniques or to pseudo-experimental techniques.

In any event, the experimental design and the instrumentation used must be viewed as critically as any other part of the inquiry, as Arthur S. Eddington has pointed out in a parable about fish nets. In his parable an ichthyologist interested in exploring ocean life uses a net with two-inch openings. After carefully surveying his catch, he concludes that no sea creature is less than two inches long. In reply to criticisms that his net was not adapted to survey all ocean life, the ichthyologist says: "Anything uncatchable by my net is *ipso facto* outside the scope of ichthyological knowledge, and is not part of the kingdom of fishes which has been defined as the theme of ichthyological knowledge. In short, what my net can't catch isn't fish." *(The Philosophy of Physical Science*, Cambridge, Cambridge University Press, 1939, pp. 16-17.)

Additional Historical Examples. Able scientific workers sometimes do not give an accurate account of the procedures they actually used, or they may believe that they are using one set of procedures but actually are using another, or they may deny flatly they are using the procedures that they are using. Our description of modern inquiry in the previous section was based on successful examples of scientific work. Three additional examples will be considered as further illustrations.

(1) For a considerable period, the spontaneous generation of life was widely accepted and used to account for many observable processes. Fermentation, for example, seems to start from non-living materials and yet exhibits life-like traits. The scientific issues were often intermixed with other issues. Some philosophic materialists favored a type of spontaneous generation of life from non-living matter in order to avoid theological ideas about the divine creation of life. Some religiously oriented scientists disapproved of spontaneous generation in order to support a religious view that life only comes from life. Some chemists, who were seeking to avoid importing biological notions into chemistry, favored spontaneous generation.

Although in 1765 Spallanzani showed that fermentation processes could be delayed indefinitely by boiling, some scientists of the period believed that they had demonstrated the occurrence of spontaneous generation. A limiting factor in scientific progress was the inadequacy of the microscope lenses then available for observing micro-organisms. In the 1830's and 40's considerable progress was made toward more useful views, but by and large spontaneous generation was still accepted and any organisms observed were believed to be *results* of putrefaction, infection, etc., rather than causes.

In 1861 Pasteur's work helped lead to the abandonment of the doctrine of spontaneous generation. Using a flask with a long, thin neck, he boiled meat broth until no bacteria remained. The neck of the flask was so narrow that new bacteria could not enter. The boiled broth could be kept for an indefinite period without putrefaction. After breaking off the neck, however, within a few hours the meat began to decay and micro-organisms were observed in the broth. To show that micro-organisms could be carried in the air, Pasteur passed air through two sterile filters and found that the first filter, but not the second, could induce putrefaction. (The account just summarized is taken from H.T. Pledge, *Science Since 1500*, London, His Majesty's Stationery Office, 1947, pp. 163-165.)

Pasteur's work, of course, did not absolutely disprove spontaneous generation, but instead developed a highly warranted description of what happens under specified circumstances, and thus made the invoking of spontaneous generation "explanations" pointless for the type of situation under investigation.

(2) Galileo performed some early experiments on the weight of air. He first worked with a large glass bottle stoppered by a leather valve. The valve was constructed to admit a syringe with which Galileo forced air into the bottle; he estimated he could force in two or three times more air than the bottle ordinarily contained. Then, using a balance with sand as the weights, he weighed the compressed air. After opening the bottle, he reweighed it and concluded that the "extra" compressed air was equivalent in weight to the sand removed from the balance. But he had not been able to measure with any accuracy the volume of the "extra" air forced into the bottle. So Galileo then devised another experiment in which he forced water into a bottle of air without allowing any air to escape; the air compressed enough so that the bottle could be filled about three-fourths full of water. The bottle containing the water plus the compressed air was weighed. Then the "extra" air was released, and Galileo concluded that the volume of air which escaped just equalled the volume of water in the bottle. Then the bottle was reweighed, with the difference between the two weighings equalling the weight of a volume of air equivalent to the directly measurable volume of water in the bottle. (Galileo, *op. cit.)*

(3) In the 18th century heat was generally believed to be a material substance in the form of a very light or weightless fluid; a hot body was said to contain more caloric fluid than a cold body. The caloric theory fit in with other prevailing notions that emphasized the presence or absence of some material fluid. Living organisms were believed to contain vital spirits, magnetized objects to contain magnetic fluid, etc. Such fluids were said to be "subtle" and capable of penetrating bodies having no visible apertures. A satisfying report that seemed to offer an "explanation" of observed changes (heating/cooling, magnetizing/demagnetizing, etc.) was thus developed that obstructed inquiry for some time to come.

In the late 1790's Rumford's work in the Munich military arsenal provided a much superior description of certain heat phenomena and showed the irrelevance of caloric theory to the warranted assertions he developed, even though the caloric theory was retained by many until the middle of the 19th century.

Rumford carefully measured the weight of gold heated from 32°F to a bright red, and found the weight did not change by one-millionth part. Inasmuch as the prevailing view was that caloric is imponderable, those measurements were consistent with the existence of caloric. Rumford's measurements, however, in conjunction with his other work, helped to show that warranted assertions about heat could be made without reliance on caloric fluid notions. In boring cannons in the arsenal, Rumford noted the considerable rise in temperature of the brass barrels and the even higher temperature of the chips being drilled out. One conjecture was that perhaps the total heat produced comes from the chips. Rumford argued that if so, according to accepted caloric notion, the "capacity for heat" of the chips should be significantly changed. But comparative measurements of those chips and of similar pieces of metal showed no such change.

He then designed an apparatus in which a hollow cylinder was enclosed in a box of water. A blunt drill was rotated continuously against the bottom of the cylinder. After one hour, the water temperature rose to 107°F; after two and one-half hours, the water boiled. Rumford concluded that the heat generated by friction appears to be inexhaustible. If the friction apparatus is isolated from other bodies and can continue to yield heat indefinitely, Rumford argued, heat could not profitably be regarded as a material substance. The only conjecture he regarded as likely, in view of his experiments, was that heat is motion. ("An Inquiry Concerning the Weight Ascribed to Heat," and "An Inquiry Concerning the Source of the Heat which Is Excited by Friction," *Philosophical Transactions of the Royal Society of London*, 1799 and 1798.)

Belief in caloric could still be maintained, of course, if one assumed that the caloric fluid flowed from the air surrounding Rumford's isolated friction system, so further work would be necessary (such as performing the experiment in a vacuum) to refute all variants of the caloric conjecture, but experiments like Rumford's helped to show both that the caloric conjecture was not useful and that a motion conjecture was useful.

Several points are suggested by the materials just discussed. At times religious, philosophical, and other beliefs can have a strong influence on the initial conjectures a scientist develops. Such beliefs can be productive, but also can be obstacles if adhered to tenaciously without sufficient, or any, evidence. Technological factors may play a significant role; at any given time the equipment necessary to test competing conjectures may not be available. Many rejected conjectures are not completely refuted, but rather are abandoned when they fail to yield useful results. Plausible "explanations," however intellectually satisfying, are simply no substitute for warranted assertions from the point of view of facilitating effective prediction and control.

D. MODERN INQUIRY INTO HUMAN BEHAVIOR

Our view is that the same general procedures so effectively applied in the physical and physiological areas of inquiry probably will lead to similarly useful results in inquiry into human behavior. That application faces at least two major initial obstacles.

(1) In Western civilization, the belief is deeply intrenched that human behavior (or some parts of that behavior) must be fundamentally different in kind from other processes we encounter, and that therefore some different method must be used. Refutation of such views consists chiefly in developing warranted assertions about human behavior that foster prediction and control. The results of inquiry into human behavior to date do not begin to compare with the results already obtained in the other areas of scientific inquiry. We suggest that the relative paucity of results is partly attributable to the non-use of the method we here call modern inquiry.

(2) The relative paucity of results is also partly attributable to a serious terminological problem. We have little in the way of agreed upon firm names useful for communicating about aspects and phases of human behavior. Until naming is improved we may remain bogged down in such confusion (of ourselves and others) and incoherence that communication is difficult or impossible.

The emphasis on useful naming sometimes is quickly dismissed as a fixation on the trivial or the merely verbal and a neglect of "substantive" problems. In our opinion, what is distinctively human about human behavior is—or involves—what we call sign-behavior. In that sense, a concern about language is a "substantive" issue in inquiry into human behavior. Moreover, we believe that in the physical and physiological areas progress was also dependent on developing useful naming. Pledge, for example, notes:

> "The French chemical world did not abandon phlogiston until about 1787; while Priestley, Scheele, Cavendish, and many others never did abandon it. The supersession of phlogiston by oxygen, together with the accumulation of what are now seen to be antique errors, *drew attention to a matter of primary importance in chemistry as in every science, nomenclature. The technical terms of the time made accurate thinking almost impossible.*" (Pledge, *op. cit.*, p. 116, emphasis added.)

This is not to imply that some scientific areas have satisfactorily resolved all the language problems; far from it. We are only suggesting that overall the communication problem is worse in inquiry into human behavior than in other areas of scientific investigation. Elaborate speculative or technical constructions are often developed, with no one being able to communicate clearly or definitely what is being talked about.

A criticism of some economists by Joseph Mayer is applicable to discussions in many areas of human behavior. In describing the quest of certain economists for a particular type of cost measurement, Mayer says:

> "Since the actual problem is so disheartening, a shell-game attempt to escape it has been inaugurated. . . . This procedure seems not only to have given no aid whatever toward a solution but to have kept the minds of economists from the actual problem. In trying to discover under which shell the cost or utility button has been put, some economists have been lulled into believing that they have been investigating the nature of the button itself. . . . [T]he mind is kept occupied with guessing under which shell the button is. . . , [but] the button itself never appears to have been laid bare for examination." *(Social Science Principles in the Light of Scientific Method*, Durham, Duke University Press, 1941, p. 95.)

"Behavior" is used here as a name covering all the adjustmental processes of organism-in-environment. This differs from those who restrict "behavior" either to muscular and glandular activities within the organism, or to those who restrict "behavior" to so-called gross, overt activities as contrasted with "inner" or "mental" activities. Man is viewed here as a biosocial organism; that is, as a biological organism operating *in-and-by-means-of* a social environment.

Knowing, as human behavior, extends over a broad range of activities. Naming is part of the range of knowing behavior. One who names states, relates, selects, identifies, orders, systematizes, etc., as aspects and phases of things and events are differentiated. Names are *not* taken as things separate from, and intermediate between, the organism and its environment. Nor do we accept the conventional sharp separation between a word and its meaning, for making the word, the word-user, and the word's meaning into separate and independent "reals" is only too likely to result in hypostatization and insoluble epistemological problems about the locus and status of "meanings" and "knowledge." Nor do we imply the slightest finality in naming; as inquiry progresses we find changes and improvements in earlier naming.

Within naming, some names are considerably more useful than others. "Specifying" is that part of naming behavior concerned with increasingly useful differentiating among aspects and phases of things and events, and more consistently accurate naming of them, as is typically found in successful scientific inquiry. Even the best scientific specifications are not taken as final. All aspects of scientific achievement are subject to possible change, correction, improvement, or abandonment.

"Describing" is expanded naming of more than one aspect or phase of some thing(s) or event(s), including relations among them. Useful descriptions are a primary aid in communication among humans and in one's own reflections. Much scientific inquiry is directed toward the development of more and more complete and useful descriptions. Descriptions often are linked together in an "If. . .then. . ." form in warranted assertions and provide humans with the information necessary to predict and control events within or without the body, including adjustive behavior to events beyond our control.

As tentative descriptions are developed in inquiry about uniformities of relationships or patterns of connections, the adequacy of the descriptions are tested by means of measured changes occurring among things and events. Logical implications of the conjectures lead to still further measurements of changes. Frequently, mathematical symbols are used to state the conjecture in shorthand form, to develop its logical implications, and to record the relevant data. Provided that the situation being investigated is such that mathematical transformations are applicable, those transformations may greatly facilitate the development and testing of conjectures.

In the history of inquiry we find three *general* procedures of inquiry, which (following Dewey and Bentley) we call the *self-actional*, the *interactional*, and the *transactional.*

In self-actional procedures, independent powers, actors, or causes are assumed to function on their own. A primitive example is describing thunderbolts as a consequence of Zeus' anger. Still prominently on display are attributions of human behavior to the free action of a mind.

In interactional procedures, presumptively independent things come together in causal interconnection. Within a field of activity, isolated elements, substances, units, or constituents of some type are assumed to have an independent existence and then to interact with each other. Examples are the severing of a word's meaning from the functioning of the word in human behavior, and accounts of behavior in which selves are said to interact with other selves or with a detachable environment.

In transactional procedures, "transaction" is the name for the full ongoing process in a field where all aspects and phases of the field, including the inquirer himself, are in common process. In many typical inquiries into human behavior the attempt to isolate and detach separate "reals" and then somehow bring them together yields incoherence, confusion, and an inability to maintain consistently the interactional procedure.

To illustrate, conventionally in inquiry into perception, "stimulus" and "response" are viewed interactionally. And yet the attempts to state just what the stimulus is, as severed from the response, and what both are in relation to other aspects and phases of the situation, often fail utterly. The following quotation shows the kind of difficulty encountered:

> "*Stimulus:* A key concept, or rather set of concepts, used in a great many ways in psychology and within the pages of this book. For purposes of this chapter, the term refers to a unit of sensory input—for example, some definable unit of light falling on and exciting the receptors in the retina of the eye. Even within this narrow definition, however, complexities inevitably arise in specifying the unit.
>
> "The unit of input may be defined by the environmental object that serves as the immediate source of energy, as in the statement: 'The stimulus was a red circle three inches in diameter.' But defining the object clearly does not specify what happens in the eye, for any environmental object can project an infinite variety of patterns onto the retina, and various objects can produce identical projections. Furthermore, as we shall see, the unit as experienced by the observer is something else again, and cannot be inferred directly from the environmental object *or* the retinal pattern. (For example, a man may 'see' his wife or 'hear' footsteps when neither would be inferred by other observers.)" (Bernard Berelson and Gary A. Steiner, *Human Behavior: An Inventory of Scientific Findings*, New York, Harcourt, Brace & World, 1964, pp. 87-88.)

Here, in the space of a few lines, we see that even within the authors' "narrow definition," the unit of sensory input referred to may be an "environmental object," or a "sensory event," or the perceiver's experiencing of something as a unit. The apparently straight-forward and common-sense sharp separation of stimulus and response simply breaks down in inquiry when one attempts to say what the two are and how they are differentiated from each other and from other parts of the situation. This is not to say that what we start out with as stimulus is *identical* to what we start out with as response, but rather that focusing attention on the full process in a field in which the aspects and phases are in common process avoids the entanglement Berelson and Steiner describe.

In an effort to avoid such entanglements, we suggest it is useful to begin by seeking agreement on names for things and events we can observe, so that we do not always have to repeat a longer description.

If we use "cosmos" to name the sum total of things we can see, taste, hear, feel, and smell (often aided by instruments), including relationships among those things, we can then differentiate among the many things in cosmos the living things, which we name "organisms." Among the organisms, we differentiate further and use the name "man" for ourselves, our ancestors, and our progeny.

In observing the transactions of man with other aspects and phases of the cosmos, we note the transactions named "eating," "breathing," etc. We also observe transactions typical of man but found infrequently or not at all in other organisms. Such transactions involve processes in which something stands for or refers to something else. We name these processes "sign-behavior" or "sign" for short.

Sign-behavior is the type of transaction that distinguishes some behavioral from physiological processes; i.e., a knowing transaction from eating, digesting, etc. No ultimate or absolute separation is suggested, for sign processes always include physiological processes and may affect them, as when the reading of a message containing bad news affects respiration. We use "sign process" as the name for the whole process, not as the name of one aspect only. "Sign" is sometimes used to refer to the thing that stands for something else (smoke as a sign of fire), but we use "sign" as short for "sign process" and as including the full situation.

Sign processes include signaling (as in beckoning, frowning, etc.), naming (as found generally in speaking and writing), and symboling (as in symbolic logic and mathematics). Within naming, we differentiate cueing (cries, expletives, interjections, etc.), characterizing (everyday practical naming), and specifying (the increasingly useful differentiating and naming of aspects and phases of things and events).

Modern inquiry (sometimes labeled scientific), then, is human knowing behavior. Human behavior is not separated from the rest of the cosmos and is investigated by the same method as used for nonhuman things and events. The central role of sign processes in human behavior adds to the importance of adequate communication when human behavior is the subject matter of inquiry.

We make no claims to originality for the views on inquiry in this chapter. Many workers have made many of the same key points. For example, the 19th century English mathematician and philosopher of science, W.K. Clifford, said in his address to the British Association in 1872:

> "Remember, then, that [scientific thought] is the guide of action; that the truth which it arrives at is not that which we can ideally contemplate without error, but that which we may act upon without fear; and you cannot fail to see that scientific thought is not an accompaniment or condition of human progress, but human progress itself." ("On the Aims and Instruments of Scientific Thought," contained in Leslie Stephen and F. Pollock, eds., *Lectures and Essays*, London, Macmillan, 1879.)

Charles Peirce and William James also helped to develop some of the main points we have made. The major contribution to date has been made by John Dewey and Arthur F. Bentley. In many respects their work is still far ahead of other work in the field, and assimilation of their achievements is far from being achieved. (See Dewey, *Logic: The Theory of Inquiry*, New York, Holt, 1938; Dewey and Bentley, *Knowing and the Known*, Boston, Beacon Press, 1949, paperback ed., 1960; Bentley, *Inquiry into Inquiries*, Sidney Ratner, ed., Boston, Beacon Press, 1954; and Sidney Ratner, Jules Altman, and James Wheeler, eds., *John Dewey and Arthur F. Bentley: A Philosophical Correspondence, 1932-1951*, New Brunswick, Rutgers University Press, 1964.)

E. SUMMARY

During man's long history, many different procedures of inquiry have been used. Many inquirers seek plausible, intellectually satisfying reports, and are confident that at least some correct results can be achieved through ratiocination alone, through an appeal to intuition, etc. But such procedures repeatedly have led to confusing, incoherent, and conflicting recommendations about solutions to problem situations.

We have attempted to describe more useful procedures of inquiry, the outcome of which is the development of warranted descriptions of things and events, including the connections among them. Those procedures, as developed to date, may be summarized as follows:

Knowing as human behavior extends over a broad range of activities.

Naming is the name applied to part of this broad range of human behavior.

Humans behaving within the naming range of knowing apply names to aspects and phases of things, including events.

Specifying is the name applied to part of naming behavior; that part concerned with increasingly useful differentiating among aspects and phases of things including events, and more consistently accurate naming of them.

Describing is the name applied to expanded naming, that is, to naming more than one aspect or phase of some thing(s) or event(s), including connections among them.

Mathematical symboling as used in knowing behavior is shorthand specifying. For example, the symbol *2* can be used as the shorthand name for two similar aspects or phases of things or events, or as the shorthand name for second in some rank-ordering of aspects or phases.

As yet, man has not succeeded in differentiating *all* the aspects or phases of any thing or event. Therefore, all naming involves what is commonly named "abstracting." Even the most accurate specifying and the most extensive describing possible today should not be assumed to be final and complete in the sense of having named or described all that ever can be known concerning any thing or event.

The procedures of inquiry include measuring changes and developing tentative conjectures, sometimes called hypotheses, about the uniformities and discrepancies found among measured changes. Such conjectures suggest additional measurements of changes, which in turn may suggest modification of earlier conjectures. When confirmed by sufficient measurements of changes, conjectures are finally accepted as theory; that is to say, as adequate descriptions of what happens under certain circumstances.

Modern inquiry, sometimes labeled scientific, is human knowing behavior, largely in the naming range of knowing, concerned with describing what happens, has happened, or might happen under specified circumstances in order to facilitate useful control over future events when practicable, including adjustive control of human behavior to external events.

The following stages occur in the course of inquiry:

1. The inquirer becomes aware of a problem situation.
2. He observes some facts that appear to be pertinent. Various aspects and phases of the situation are differentiated, some changes among them are measured, and a tentative partial description of what is happening is begun.
3. In noting connections among some of the things observed and measured, other connections may be

imagined. The inquirer focuses on what seem to be the pertinent aspects and phases of the situation, and develops a conjecture as to what may happen under specified circumstances.

4. That conjecture may involve other facts to be observed, perhaps including some facts originally not believed to be pertinent. As the inquirer proceeds, he may find that the original problem situation is quite different than it first had seemed.

5. The tentative description of what happens is supplemented and perhaps revised. Transformations via verbal or mathematical logic may be used. What were earlier taken as facts may be revised or rejected.

6. Perhaps another conjecture occurs to the inquirer about possible connections among facts, including measured changes.

7. Investigation of the new conjecture requires further observation and perhaps results in the development of a more adequate description. These procedures of observation, reconsideration, renewed observation; i.e., the interweaving or reciprocal stimulation of what are sometimes called empirical observations and the formulation of hypotheses, may be repeated many times in succession.

8. Finally, if the inquirer is successful, a description adequate for resolving the immediate problem situation is developed.

9. Further inquiries may further supplement the description of what happens; in some instances new inquiries may reveal aspects or phases that force drastic amendment of the best earlier description.

10. Inquiry has no necessary end. A *complete* description of even a simple problem situation apparently never has been achieved and may never be, but an adequately useful description is the goal of modern scientific inquiry.

II.

PSYCHOLOGY*

1. WORKING DESCRIPTION OF THE FIELD

PSYCHOLOGISTS inquire into human and animal behavior, with emphasis on both the individual's adjustive procedures and the evolution of those adjustments within a species. Behavioral processes such as learning, speaking, perceiving, etc., are investigated, with considerable attention given to the measurement of individual differences in those processes. On the one side, psychological inquiry merges with physiological and other biological inquiry; on the other, with inquiry into group behavior.

2. OTHER DESCRIPTIONS OF THE FIELD

Modern psychology had its inception in Germany with Gustav Fechner, Hermann Ebbinghaus, and Wilhelm Wundt. In America psychology began with William James' establishment of a laboratory at Harvard. James described psychology as "the science of mental life, both of its phenomena and of their conditions." *(The Principles of Psychology*, New York, Holt, 1890.) Understood earlier as "the science of consciousness," in the twentieth century the field was viewed by most psychologists as "the science of behavior." John Watson said that psychology "is a purely objective, experimental branch of natural science." ("Psychology as the Behaviorist Views It," *The Psychological Review*, Vol. 20, 1913, p. 176.)

Some commentators emphasize the behavior of the *individual* as the main object of study. M. Brewster Smith says that in psychology "what runs through an otherwise heterogeneous history is a pervading focus on the individual." ("Anthropology and Psychology," in John Gillin, ed., *For a Science of Social Man*, New York, Macmillan, 1954, p. 33.) And Walter Reitman describes psychology as the study of the "individual in interaction with his environment." ("Psychology," in Bert F. Hoselitz, ed., *A Reader's Guide to the Social Sciences*, rev. ed., New York, Free Press, 1970, p. 93.)

Others maintain that social behavior is also part of psychological inquiry. In any event, a total separation of the individual and the social does not seem possible. As B.B. Wolman points out, even "purely" physiological events involve an environment. *(Contemporary Theories and Systems in Psychology*, New York, Harper, 1960.) And Hadley Cantril argues that there can be "no 'person' except for an 'environment,' nothing 'subjective' except for what is 'objective,' nothing 'personal' except for what is 'social.'" ("Toward a Humanistic Psychology," *ETC.*, Vol. XII, 1955, p. 280.)

Sometimes descriptions of the field of psychological inquiry are so broad as to include all, or practically all, the behavioral fields. A special committee at Harvard, for example, said:

> "Psychology is actually what psychologists do and teach: defined briefly, it is the science of human and animal behavior, both individual and social. To expand this definition, psychology is the systematic study, by any and all applicable and fruitful methods, of organisms in relation to their behavior, environmental relations, and experience. Its purpose is to discover facts, principles, and generalizations which shall increase man's knowledge, understanding, predictive insight, directive wisdom and control of the natural phenomena of behavior and experience, and of himself and the social groups and institutions in which and through which he functions." *(The Place of Psychology in an Ideal University*, Cambridge, Harvard University Press, 1947, p. 2.)

The following quotation illustrates the wide range of contemporary psychological inquiry:

> "Our subject matter has become quite boundless: muscle twitches and wars, the sound of porpoises and problems of space, the aesthetic qualities of tones and sick minds, psychophysics and labor turnover. The range of organisms involved in the studies of these problems extends from pigeons to people, from amoeba to social groups." (Wilse B. Webb, "The Choice of the Problem," *American Psychologist*, Vol. 16, 1961, p. 223.)

A not atypical description of the field is found in the recent survey of behavioral sciences sponsored by the National Academy of Sciences and the Social Science Research Council:

> "Psychology is usually defined as the scientific study of behavior. Its subject matter includes behavioral processes that are observable, such as gestures, speech, and physiological changes, and processes that can only be inferred, such as thoughts and dreams. Psychology occupies a crucial position among the disciplines that endeavor to understand man scientifically. It is in part a life science, closely related to biology and psychiatry, for it studies the behavior and mental functions of living organisms. It is also a social science, related to anthropology, sociology, and political science, for it deals with behavior in complex social environments." (Kenneth E. Clark and George A. Miller, eds., *Psychology*, Englewood Cliffs, Prentice-Hall, 1970, p. 5.)

In recent years, however, some psychologists have begun to reject the type of scientific procedures of inquiry so successfully applied in the physical sciences. R.B. MacLeod, who urges the use of a phenomenological method, insists that "consciousness" can and should be studied, and says that to be a scientist "is to have boundless curiosity tempered by discipline." ("Phenomenology," in W.T. Wann, ed., *Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology*, Chicago, University of Chicago Press, 1964, p. 71.) Others have defended in psychological inquiry the mentalism urged by Noam Chomsky in linguistics, or have developed existentialist and humanistic approaches that are nonscientific. (See John Shotter and Alan Gauld, "The Defense of Empirical Psychology," *American Psychologist*, Vol. 26, 1971; William D. Hitt, "Two Models of Man," *American Psychologist*, Vol. 24, 1969; Abraham H. Maslow, "What Psychology Can Learn from the Existentialists," Ch. 2 of his *Toward a Psychology of Being*, New York, Van Nostrand Reinhold, 1968; and Charlotte Buhler, "Basic Theoretical Concepts of Humanistic Psychology," *American Psychologist*, Vol. 26, 1971.)

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*We are grateful to William K. Estes for his thorough criticism of the original version of this chapter. In our revision we have adopted many of his proposals and have incorporated many of his suggested wordings. However, he might well disagree with many of the judgments we have made; consequently the chapter should not be taken as representative of his views.

3. METHODS AND TYPES OF INQUIRY

A cross-sectional view of the field of psychology can be gained by looking at the principal subdisciplines that have arisen in response to different historical traditions.

(1) *Developmental and Experimental Child Psychology.* For a long period child psychologists primarily observed children to get information about the maturation of various skills and capacities. (A. Gesell and F.L. Ilg, *The Child from Five to Ten*, New York, Harper, 1946; F.L. Goodenough, *Developmental Psychology*, New York, Appleton-Century, 1934; L. Carmichael, ed., *Manual of Child Psychology*, New York, Wiley, 1954.) More recently workers in this field have combined observational, experimental, and "theoretical" procedures. (See L.P. Lipsitt and C.C. Spiker, *Advances in Child Development and Behavior*, New York, Academic Press, 1963, 1965, 1967.) Much current work is influenced by and directed by leading ideas from Jean Piaget (*The Language and Thought of the Child*, New York, Harcourt, 1926), from work in linguistics (T. Bever, "The Cognitive Basis for Linguistic Structures," in J.R. Hayes, ed., *Cognition and Language Learning*, New York, Wiley, 1970), and from developments in general learning and behavior theory[*] (W.K. Estes, *Learning Theory and Mental Development*, New York, Academic Press, 1970). Cross-cultural comparisons of cognitive development in children raised in very different circumstances is an important new trend in child psychology. (M. Cole, J. Gay, J.A. Glick, and D.W. Sharp, *Culture and Cognitive Processes*, New York, Basic Books, 1971.)

(2) *Comparative and Physiological Psychology.* For several decades following the pioneering works of E.L. Thorndike (*Animal Intelligence*, New York, Macmillan, 1911), J.B. Watson (*Behaviorism*, New York, Horton, 1925), and R.M. Yerkes ("The Mental Life of Monkeys and Apes," *Behavioral Monographs*, Vol. 3, 1916), research in animal behavior was largely concerned with intensive analyses of particular laboratory situations, such as the multiple T-maze, the problem box, or the "Skinner box," for a few selected species of animals, most often the albino rat. Relatively little attention was given to differences among species and phyla. Several general learning theories were developed, notably those of Clark L. Hull (*Principles of Behavior*, New York, Appleton, 1943), B.F. Skinner (*The Behavior of Organisms*, New York, Century, 1938), and E.C. Tolman (*Purposive Behavior in Animals and Men*, New York, Appleton, 1932). Later some of the leading ideas of the *ethologists*, notably K.Z. Lorenz (*King Solomon's Ring*, New York, Crowell, 1952) and N. Tinbergen (*The Study of Instinct*, Oxford, Clarendon, 1951), led to increased interest in analyzing differences in behavior organization among species and in studying animal behavior in closer relation to its natural settings.

(3) *General Experimental Psychology.* Sensory psychology was the first subdiscipline to develop substantial experimental and quantitative methods, beginning with the work of G.T. Fechner, H.L.F. Helmholtz, and W. Wundt. (See E.G. Boring, *A History of Experimental Psychology*, 2nd ed., New York, Appleton-Century-Crofts, 1950.) Much work in this tradition has remained closely aligned with physiology, leading to quantitatively stated conjectures about receptor processes (for example, J.C.R. Licklider, "Three Auditory Theories," in S. Koch, ed., *Psychology: A Study of a Science*, Vol. 1, New York, McGraw-Hill, 1959; M.H. Pirenne and F.H.C. Marriott, "The Quantum Theory of Light and the Psychophysiology of Vision," in Koch, *op. cit.*) and to the exploration of basic physiological and biochemical processes (for example, R. Granit, *Receptors and Sensory Perception*, New Haven, Yale University Press, 1955; S.S. Stevens and H. Davis, *Hearing*, New York, Wiley, 1938; C. Pfaffmann, "The Afferent Code for Sensory Quality," *American Psychologist*, Vol. 15, 1960).

Other work in sensory psychology has led to the various branches of psychophysics and, more recently, signal detectability theory. (See, for example, S.S. Stevens, "Mathematics, Measurement and Psychophysics," in S.S. Stevens, ed., *Handbook of Experimental Psychology*, New York: Wiley, 1951; J. Swets, ed., *Signal Detection and Recognition by Human Observers*, New York, Wiley, 1964.) The study of perception, originally almost indistinguishable from work in sensory psychology, has branched out into the behavioral conjectures of C.H. Graham and his associates (*Vision and Visual Perception*, New York, Wiley, 1965), the phenomenological procedures of J.J. Gibson (*The Perception of the Visual World*, Boston, Houghton, 1950), and various limited quantitatively elaborated hypotheses. Current active research fields concern perceptual learning (E.J. Gibson, *Principles of Perceptual Learning and Development*, New York, Appleton-Century-Crofts, 1969) and visual information processing (R.N. Haber, *Information Processing Approaches to Visual Perception*, New York, Holt, 1969).

(4) *Learning.* Research on learning prior to about 1940 was dominated by Pavlov's work in conditioning, by association theory as interpreted by Thorndike, and by the largely eclectic functionalist tradition as represented by R.S. Woodworth (*Experimental Psychology*, New York, Holt, 1938) and J.A. McGeach (*The Psychology of Human Learning*, New York, Longmans, 1942). During the next decade research was concentrated heavily upon animals rather than upon human subjects, and was organized and directed largely in terms of the learning theories of C.L. Hull, E.R. Guthrie, B.F. Skinner, and the controversies involving them. More recently there has been increasing attention to human memory, verbal learning, and so-called "concept" formation. (Much work on "concept" formation inquires into the application of names to what is common to a class of things, as when a young child learns that both *men* and *women* are *people.*)

(5) *Personality and social psychology.* The field of personality originally was a subfield of differential psychology, investigated either in terms of typologies or trait organizations. (See A. Anastasi and J.P. Foley, Jr.,

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[*] In discussing the historical development of psychology and related matters, for the reader's convenience we use conventional labels such as "learning theory," "Freudian theory," "Weber's Law," etc. We wish, however, to draw attention to the differing applications of "theory"; a name applied sometimes to highly speculative conjectures, sometimes to the best warranted assertions available, and sometimes to assertions intermediate between those extremes. "Law" also sometimes turns out to have been applied to unwarranted conclusions. (See Section 8 on terminology.)

Differential Psychology, New York, Macmillan, 1949.) More recently, inquirers have turned strongly toward the study of motivational processes in the individual. Major influences have been psychoanalytic theory, Kurt Lewin's topological procedures (*A Dynamic Theory of Personality*, trans. by D.K. Adams and K.E. Zener, New York, McGraw-Hill, 1935), and general behavior theory (John Dollard and N.E. Miller, *Personality and Psychotherapy*, New York, McGraw-Hill, 1950). Concurrently social psychologists have developed experimental techniques; the main lines of research concern attitudes and their determinants, thinking and knowing processes of particular relevance for interpersonal relations, and study of relations in small groups.

Schools of Psychology

During the first half of the century several distinctive schools or systems of psychology developed concerning the subject matter and objectives of psychology. (See R.S. Woodworth, *Contemporary Schools of Psychology*, rev. ed., New York, Ronald, 1948.) Many of those schools have since been abandoned (although traces persist), and in general schools of the older type no longer exist. In the sense of characteristic points of view, however, there are distinctive groupings of contemporary psychologists.

a) *Behaviorism.* Many leading American psychologists today are sympathetic to some form of behaviorism, deriving from the early version presented by John Watson in 1913:

> "Psychology, as the behaviorist views it, is a purely objective, experimental branch of natural science which needs introspection as little as do the sciences of chemistry and physics. . . .The behavior of animals can be investigated without appeal to consciousness. . . .The behavior of man and the behavior of animals must be considered on the same plane. . . ." (Watson, 1913, *op. cit.*, p. 176.)

Watson was interested in giving a motor or physiological account for all psychological behavior. His position is sometimes called "mechanistic behaviorism" and has some similarities to Ivan Pavlov's "reflexology." Another type of behaviorism goes still further. The "physicalists" search for physicochemical bases for human behavior and believe that eventually psychology will become part of physics (i.e., the "laws" of psychology will be reducible to physical "laws," or the "language" of physics will suffice to "explain" psychological phenomena, or that in some other way behavior will be fully describable in physical terms).

Many recent behaviorists take a broader view. Edward C. Tolman preferred the label "purposive behaviorism" and emphasized inquiry into goal-directed behavior, such as that of an animal in a maze. He argued that the use of "intervening variables" makes possible the description of behavior in terms of functions that are not themselves directly observable.

Clark L. Hull championed hypothetical-deductive procedures allowing for experimental testing. (Hull, *op. cit.)* Although much criticized, his work is often viewed as an excellent example of theory construction in psychology. Burrhus F. Skinner remains the outstanding exponent of "radical behaviorism" and "operationism." (See *Verbal Behavior*, New York, Appleton-Century-Crofts, 1957; "Behaviorism at Fifty," *Science*, Vol. 140, 1963.) Most recent behaviorists are willing to make use of introspective data, as long as the resulting hypotheses are testable, directly or indirectly.

Edwin G. Boring summarizes a widely held viewpoint:

> "The fundamental principle here is that science deals only with public information, and, whatever private consciousness may be like, it gets into science only by publication of some sort—by the words or gestures or other behavior of the organism to whom the consciousness belongs. Even the unconscious is tapped by the psychoanalysts only through the use of words or other behavior. This bit of behavioristic logic put the American psychologists at ease, allowing most of them to go all out for behaviorism." ("Psychology," in James R. Newman, ed., *What is Science?*, New York, Simon and Schuster, 1955, pp. 300-301.)

Behaviorism, in extreme or mild versions, has been challenged from various points of view. Some recent alternatives to behaviorism are gestalt psychology, psychoanalysis, existentialist and phenomenological psychology, humanistic psychology, and transactional psychology.

b) *Gestalt psychology*. Gestalt psychologists such as Max Wertheimer (*Productive Thinking*, New York, Harper, 1945), Wolfgang Köhler (*Gestalt Psychology*, New York, Liveright, 1929), and Kurt Koffka (*The Growth of the Mind*, translated by R.M. Ogden, London, Kegan Paul, 1924) denied that psychological processes are composed of discrete elements that can be discovered by a reductive analysis, and focused on dynamic processes of integration and selective organization. Kurt Lewin's work on "life space" and "field theory" generated considerable discussion. (See his "Behavior Development as a Function of the Total Situation," in Carmichael, *op. cit.*)

The language used by the gestalt psychologists to describe their "holistic" methods was sometimes obscure; considerable emphasis was given to phrases such as "the whole is greater than the sum of its parts." Gestalt inquiry may be more adequately described as inquiry that focuses attention primarily on relations among things constituting a presumed whole, as contrasted with focusing attention on the aspects and phases of differentiated parts. Functioning systems and subsystems are the units observed and described, rather than the components of those systems. Although there has been much criticism of gestalt speculation, the experimental work in perception done by gestalt psychologists is useful and highly regarded.

c) *Psychoanalytic psychology*. Many psychological inquiries are concerned primarily with activities and processes characteristic of thinking and knowing behavior, believed to be common to all normal individuals, but psychoanalysts emphasize the motivational determinants of behavior and deviations from the norm ("psychopathology"). The term "psychoanalysis" is used in several ways. It may refer to Sigmund Freud's insistence that repressed factors in the unconscious account for much behavior, to his techniques for investigating such phenomena, to his treatment of his patients, or to the general procedures characteristic of Freud and his followers.

For Freud human behavior was influenced by conflicting impulses of love and hate, sexual motivation, the "id," "libido," etc. The motives of human behavior, he maintained, are biologically generated and undergo progressive changes as a result of social pressures, especially in the early years of life as the "superego" develops. These processes result in rivalries between children and parents, between siblings, and so on, and may lead to neurotic reactions such as substitution, frustration, regression, and fixation. Neurotic behavior patterns can sometimes be overcome with great effort, but only if the person becomes aware of and adjusts to his "true" situation. In Freud's therapeutic method, the patient's free association and his dreams, humor, slips-of-the-tongue, etc., are interpreted in order to uncover unconscious motives and conflicts.

Many later psychoanalysts deviated from some of Freud's views and interpretative principles. Alfred Adler founded "individual psychology," a branch of psychoanalysis that emphasized striving for power and its role in personality development. Carl G. Jung emphasized the "collective unconscious" and "archetypes" as part of a biological inheritance. Karen Horney advocated the importance of "self-development" in relationships with others. Erich Fromm stressed social, economic, and cultural factors, and Harry Stack Sullivan adopted "interactional" procedures. Ruth Monroe's *Schools of Psychoanalytic Thought: An Exposition, Critique, and Attempt at Integration* (New York, Dryden, 1955) provides a useful account of the psychoanalytic schools.

Many scientifically oriented inquirers were severely critical of psychoanalytic conjectures, not least because those conjectures characteristically were stated in ways that were not subject to empirical testing. Recently there have been attempts to reformulate psychoanalytic principles in empirically testable ways. (See especially Albert Ellis, "An Operational Reformulation of Some of the Basic Principles of Psychoanalysis," in Herbert Feigl and Michael Scriven, eds., *Minnesota Studies in the Philosophy of Science*, Vol. 1, Minneapolis, University of Minnesota Press, 1956; B.F. Skinner's critique of traditional psychoanalysis, *Ibid.*; and R.R. Sears, "Experimental Analysis of Psychoanalytic Phenomena," in J. McV. Hunt, ed., *Personality and the Behavior Disorders*, Vol. 1, New York, Ronald, 1944.)

d) *Existential and phenomenological psychology*. The outgrowth of existentialism from phenomenology in philosophy has been paralleled in psychology by phenomenological and existentialist procedures of inquiry. For present purposes those developments can be grouped together. Psychologists supporting this general tendency oppose the "objective" procedures of scientific inquiry and emphasize the inner, subjective world of the individual. According to Donald Snygg: "Behavior is completely determined by and pertinent to the phenomenological field of the behaving organism. By phenomenological field is meant the universe, including himself, as experienced by the behaver at the moment." ("The Need for a Phenomenological System of Psychology," *Psychological Review*, Vol. 48, 1941, p. 411.)

Frequently human nature is regarded as unique, and man is viewed as having free will. Some existentialists emphasize the almost total freedom of the "authentic man" from environmental and even genetic causes and conditions. (See Rollo May, E. Angel, and H.F. Ellenberger, eds., *Existence: A New Dimension in Psychiatry and Psychology*, New York, Basic Books, 1958; MacLeod, *op. cit.*; Hitt, *op. cit.*; and Carl R. Rogers, *Client-Centered Therapy*, Boston, Houghton Mifflin, 1951.)

e) *Humanistic psychology*. Humanistic psychology is closely related to existentialistic and phenomenological psychology. Charlotte Buhler says that "existential considerations form the underlying philosophical basis of humanistic psychology," and that to "help the person experience his existence as real is the essential goal of the humanistic psychotherapist." (Buhler, *op. cit.*, p. 380.)

The late Abraham H. Maslow was the leading proponent of humanistic psychology, which he saw as a "third force" that would build on, but surpass, the viewpoints of behaviorism and psychoanalysis. In Maslow's opinion, the psychoanalytic psychologists derived their notions of human potentiality from a study of poorly functioning or pathological people, while the behaviorists overemphasized statistical averages in discussing "normal" behavior and minimized the norms of successfully functioning humans. In contrast to those views, Maslow advocated inquiry into healthy, self-actualizing humans, such as "saints, sages, good leaders, responsibles, . . .strong men, winners rather than losers, constructors rather than destroyers. . . ." *(Toward a Psychology of Being*, 2nd ed., New York, Van Nostrand Reinhold, 1968, p. iv.) Maslow's work had considerable appeal for many psychologists (especially clinical and applied workers) who believed academic psychology was becoming increasingly irrelevant, and he was elected President of the American Psychological Association for 1967-1968.

In his hierarchy of needs, Maslow differentiated between *basic (or deficiency) needs*, which include physiological needs and needs for safety and security, love and belongingness, self esteem and esteem of others, and *growth (or Being or meta-) needs*, such as meaningfulness, self-sufficiency, justice, truth, goodness, beauty, etc. In a social setting in which the basic needs are easily gratified, the emphasis can be placed on the meta-needs; Maslow believed that under those circumstances the good of the individual and the social good "can come closer and closer to being synonymous rather than antagonistic." *(Ibid.*, p. 221.)

Maslow also believed that a normative psychology was possible in which a study of healthy people would reveal the values that should be accepted; there is a scientific basis for moral principles and responsible behavior. (For a recent account of Maslow's work and its extension to "responsibility psychology," see Frank Goble, *The Third Force*, New York, Pocket Books, 1971.)

f) *Transactional psychology*. Stemming from *Knowing and the Known* by John Dewey and Arthur Bentley (Boston, Beacon Press, 1949, paperback ed., 1960), transactional psychology contrasts markedly with phenomenology and existentialism. Transactional psychologists reject self-action and interaction for the view "that man's thought and behavior can be understood only as processes of a 'full situation of organism-environment.' " (Hadley Cantril *et. al.*, "Psychology and Scientific Research, III: The Transactional View in Psychological Research," *Science*, Vol. 110, 1949, p. 517. See also Franklin P. Kilpatrick, ed., *Explorations in Transactional Psychology*, New York, New York University Press, 1961.) Transactionalists emphasize both the alteration the organism incurs in transacting with other aspects of the environment and the extent to which an individual's perceptions are influenced by his past experience.

A similar, "biosocial," point of view about psychological inquiry has been developed by Norman Cameron. Quoting him at length will illustrate many current methodological issues:

> "We have already wasted years of effort in trying to work out the internal structure of a fictitious psyche and its esoteric love life, when we might better have been working out the dynamics of the

organization, disorganization and reorganization of human behavior—of action and reaction, of thinking, wishing, loving, hating, learning, fearing, forgetting, avoiding, desiring and hiding—but all these studied as the activities of a social organism, not the dreams of a ghost. . . .This is essentially the *biosocial* point of view. It differs radically from the contemporary *psychosomatic* approach to the behavior disorders by breaking completely with the tradition of mind-body dualism. There is no need to begin by accepting the ancient and gratuitous assumption that an invisible and intangible psyche lurks within the soma, or is coextensive with it. We begin instead with what we find, a biological organism operating in and by means of a social environment. We thus create no artificial need to solve such meaningless conundrums as *How does the soma affect the psyche? How does the psyche influence the soma? And how is nonpsychic reality ever contacted and tested by an insubstantial psyche?* These questions are not inherent in the problems which our patients present. They are the offspring of psychosomatic dualism and we can discard them with their parent.

"The point of view which I have developed in this work differs from classical *behaviorism* in rejecting reflexes, instincts and emotions as building blocks out of which human behavior was supposed to be constructed. It is holistic and analytical rather than atomistic and synthetic. . . .The biosocial interpretation departs from traditional *psychobiology* by dispensing entirely with the concept of consciousness and the distinction between mental and nonmental." (*The Psychology of Behavior Disorders*, Boston, Houghton Mifflin, 1947, Preface.)

Types of Specific Methods Used

Psychologists have devoted considerable effort to the development of suitable experimental, quantitative, and observational methods. An account of the major methods follows.

(1) *Genetic*[*] *methods.* From the beginnings of psychology as a separate discipline, a major cluster of issues has centered around the problem of analyzing the contributions of heredity and environment to behavior. Initially, relevant information came largely from family history studies, an important refinement of which is the method of co-twin control utilized in efforts to ascertain the relation of genetic factors to individual differences in intelligence and personality. (H.D. Carter, "Ten Years of Research on Twins," in *39th Yearbook of the National Society for the Study of Education*, 1940.)

These techniques have been supplemented to an increasing extent by selective breeding studies with lower organisms, beginning with relatively crude efforts of R.C. Tryon and others to develop "maze bright" and "maze dull" strains of rats ("Genetic Differences in Maze-Learning Ability in Rats," in *39th Yearbook of the National Society for the Study of Education*, 1940), and evolving later into more elaborate analyses of genetic components of variance in simple behaviors of lower organisms (J.L. Fuller and W.R. Thompson, *Behavior Genetics*, New York, Wiley, 1960).

(2) *Longitudinal methods.* Originally simply observation of children over considerable periods of time in order to gain information about the maturation of various capacities, the longitudinal method has become refined in many respects. Field observations of behavior are often supplemented by systematic psychological testing at various ages, as in L.M. Terman's studies of gifted children. (*Genetic Studies of Genius*, Stanford, Stanford University Press, 1925.) Scales have been developed that facilitate quantitative descriptions of the development of both physical skills and intellectual capacities. (N. Bayley, "Mental Growth in the First Three Years," *Genetic Psychology Monographs*, 1933, Vol. 14; J. Kagan & H. Moss, *Birth to Maturity: A Study in Psychological Development*, New York, Wiley, 1962.) During the past few years investigators have become increasingly concerned with designing experiments to trace developmental changes. (See Estes, 1970, *op. cit.*; Lipsitt and Spiker, *op. cit.*)

(3) *Psychological measurement.* The concern with measurement in psychophysics has led to theories and techniques of scaling, with attempts to make relatively precise measurements of sensory experiences and of more complex behaviors (J.P. Guilford, *Psychometric Methods*, New York, McGraw-Hill, 1954; L.L. Thurstone and E.J. Chave, *The Measurement of Attitudes*, University of Chicago Press, 1929), and, through the early work of J. McK. Cattell and E.L. Thorndike (*The Measurement of Intelligence*, New York, Teachers College, 1925), to the development of psychological tests widely used in applied psychology (L.J. Cronbach, *Essentials of Psychological Testing*, New York, Harper, 1949). Associated with the development of test theory are methodological procedures for dealing with such characteristics as the reliability and validity of tests and the development of norms. (H. Gulliksen, *A Theory of Mental Testing*, New York, Wiley, 1950.)

(4) *Mathematical models.* The development of experimental methods in psychology has been paralleled by the growth and elaboration of mathematical models. The earliest models having an important role in guiding research were the psychophysical "laws" of Weber and Fechner, which led to such contemporary models as H. Helson's. (*Adaptation-Level Theory*, New York, Harper and Row, 1964.) The heavy concentration of research upon problems of intelligence and the organization of "mental abilities" during the early years of this century led rapidly to the flourishing of factor analysis, which has continued to be a technique widely applied. (L.L. Thurstone, *Multiple Factor Analysis*, University of Chicago Press, 1947.) In the areas of perception, learning, and communication, much attention has been given during the past two decades to attempted applications of information theory (W.R. Garner, *Uncertainty and Structure as Psychological Concepts*, New York, Wiley, 1962), probabilistic and statistical models, and game and decision theory (R.D. Luce, R.R. Bush, and E. Galanter, *Handbook of Mathematical Psychology*, New York, Wiley, Vol. 1, 1963; Vol. 2, 1965).

(5) *Experimental analysis.* Specific lines of development within this broad domain include operant conditioning procedures (A.C. Catania, *Contemporary Research in Operant Behavior*, Glenview, Scott-Foresman, 1968), the development of multivariate methods (R.B. Cattell, ed., *Handbook of Multivariate Experimental Psychology*, Chicago, Rand McNally, 1966), and the exploration of the neural correlates of behavior by surgical extirpation techniques and by direct electrical stimulation of the brain (S.P. Grossman, *A Textbook of Physiological Psychology*, New York, Wiley, 1967).

(6) *Field observational studies.* Field observation of both animal and human behavior has been progressively

improved with the development of photographic techniques and rating scales methods for quantifying observations.

4. RESULTS ACHIEVED

The enormous number of written reports in psychology makes summarizing difficult. Even *Psychological Abstracts* has become voluminous. In this Section we present a sampling of the type of results achieved or believed to have been achieved, classified under some conventional headings. The results noted should not be confused with the "warranted assertions" discussed in Chapter 1, although many such warranted assertions have been developed by psychologists.

Frequently in a given report we find a mixture of warranted assertions, apparently good guesses, and speculation. Moreover, many research efforts rest on prior work that may be problematic. To illustrate, reference will be made to Stanley Schacter's work on obesity. His research is impressive in many respects and may have considerable practical importance in the control of obesity. He and a co-worker decided to make a point-by-point comparison of his findings on humans with the work of others on the hypothalamic obese rat. Schacter was not a physiological psychologist, but he attempted to read everything about the appropriate rat subjects. He says:

> "If you've ever made this sort of attempt, you may have been seized by the same despair as we were when it sometimes seemed as if there were no such thing as a fact that *someone* had not failed to confirm. . . .And it sometimes seemed as if there were no such thing as an experiment which *someone* had not failed to replicate. . . .I cannot say that I find this particularly surprising, but if one is trying to decide what is the fact, it is a depressing state of affairs." ("Some Extraordinary Facts about Obese Humans and Rats," *American Psychologist*, Vol. 26, 1971, p. 132.)

Later Schacter notes that none of the existing accounts of obesity fit all the currently available facts (or what seem to be facts), and he attempts to develop a new conjecture or hypothesis that does. He also candidly discusses some of the problems his hypothesis encounters. He concludes by attempting to sort out "what I believe we now know, what seems to be good guesswork, and what is still out-and-out speculation." (*Ibid.*, p. 137, pp. 142-143.)

In what follows, then, the results that appear promising may later turn out not to be useful, and the warranted assertions achieved are intermixed with some as yet speculative material.

Sensation and Perception

The early work of Selig Hecht, followed by the contributions of G. Wald, R. Granit, and H.K. Hartline, among others, led to more adequate descriptions of the photochemistry of visual reception and other retinal processes basic to vision. (See Pirenne and Marriott, *op. cit.*) The duplicity theory of rod and cone function has become well established and helps to account for the processes of light and dark adaptation. The interpretation of color vision has advanced from Helmholtz's trichromatic theory and Hering's opponent colors theory to the quantitative theory of Hurvich and Hurvich (C.H. Graham, "Color Theory," in Koch, 1959, *op. cit.*) and the investigation by R. DeValois of the specific neural mechanisms involved in transmission of color information through several levels of the vertebrate nervous system ("Behavioral and Electrophysiological Studies in Primate Vision," in W.D. Neff, ed., *Contributions to Sensory Physiology*, New York, Academic Press, 1965). In audition, much work has been done on spatial localization, the cochlear interpretation of frequency discrimination, and the volley theory of loudness. (See E.G. Wever, *Theory of Hearing*, New York, Wiley, 1949.)

Developments have also occurred in the less frequently studied sensory areas. In the case of gustation and olfaction, the earlier preoccupation with classification of substances giving rise to different experiences has been followed by studies of neurophysiological mechanisms and their connections with motivations. (See C. Pfaffmann, ed., *Olfaction and Taste*, New York, Rockefeller University Press, 1969.) Use of systematic observational techniques indicates that infants discriminate odors at earlier ages than formerly suspected. (E.g., T. Engen, L.P. Lipsitt, & H. Kaye, "Olfactory Responses and Adaptation in the Human Neonate," *Journal of Comparative and Physiological Psychology*, Vol. 56, 1963.) The study of tactual sensation has possible useful applications in developing alternative routes of efficient information input for individuals suffering visual or auditory handicaps. (See F.A. Geldard, *The Human Senses*, New York, Wiley, 1963.)

In psychophysics, the early quantitative work leading to Weber's and Fechner's "laws" and the notion of a "just noticeable difference" has been followed by a family of methods for measuring sensory magnitudes, based on Thurstone's law of comparative judgment, Steven's power law, and Luce's choice theory (Luce, Bush, and Galanter, 1963, *op. cit.*). Gestalt approaches to perception, which led to important findings concerning figure-ground relationships and perceptual organization, have been largely superseded by more behavioral procedures (Graham, *op. cit.*) and a general theory of adaptation level (Helson, *op. cit.*). D.O. Hebb's neurophysiological theory has been a major influence in research on sensory deprivation and developmental aspects of perception. (*Organization of Behavior*, New York, Wiley, 1949.) Recent trends include the virtual replacement of classical threshold theory with applications of the theory of signal detectability (Swets, *op. cit.*), the work of J.J. Gibson on perception of surfaces (*op. cit.*), and the initiation of major research efforts on perceptual learning (E.J. Gibson, *op. cit.*), the perception of speech, and social influences on perception (F.H. Allport, *Theories of Perception and the Concept of Structure*, New York, Wiley, 1955).

Learning and Memory

I.P. Pavlov's classic work on conditioned reflexes (*Conditioned Reflexes*, trans. by G.V. Anrep, London, Oxford University Press, 1927) has been highly influential, although his specific notions have largely been superseded by J. Konorski's neurophysiological theory of conditioning (*Integrative Activity of the Brain*, Chicago, University of Chicago Press, 1967) and the behavioral theories of Hull (*op. cit.*) and K.W. Spence (*Behavior Theory and Conditioning*, New Haven, Yale University Press, 1956).

Major results include the discovery of the partial reinforcement effect (that is, increased resistance to extinction following intermittent reinforcement); the conditioned emotional response, which has served widely as a baseline measure in studies of the emotional effect in conditioning, the effects of drugs, and the like (W.K. Estes and B.F. Skinner, "Some Quantitative Properties of Anxiety," *Journal of Experimental Psychology*, Vol. 29, 1941); the phenomenon of "blocking of conditioning" by L. Kamin; demonstration of the important role of contingencies in conditioning by R. Rescorla (see N.J. Mackintosh and W.K. Honig, eds., *Fundamental Issues in Associative Learning*, Halifax, Dalhousie University Press, 1969); and work on operant behavior in relation to schedules of reinforcement (C. Ferster and B.F. Skinner, *Schedules of Reinforcement*, New York, Appleton-Century-Crofts, 1957), with its numerous applications to the shaping of behavior in practical stiuations. (In operant conditioning the subject is presented with a reward immediately following the desired behavior, as when a pigeon is taught to walk in a S-shaped path by giving it food whenever it moves in the right direction.)

The general and somewhat programmatic learning theories developed by Tolman, Hull, and Guthrie were important influences upon research in learning during the period 1930-1950. The difficulty of working with such broad systems and the criticisms they received on technical grounds (W.K. Estes *et al.*, *Modern Learning Theory*, New York, Appleton-Century-Crofts, 1955) led to the recent trend toward more limited and quantitative theories. Some of these represent specializations of aspects of Hull's theory (e.g., F.A. Logan and A.R. Wagner, *Reward and Punishment*, Boston, Allyn and Bacon, 1965) and some are variations of Hull's basic point of view, such as N.E. Miller's "liberalized association theory" ("Liberalization of Basic S-R Concepts: Extensions to Conflict Behavior, Motivation and Social Learning," in Koch, 1959, *op. cit.*). Still others, such as statistical learning theory, involve basically different points of view. (See, for example, E.C. Neimark and W.K. Estes, *Stimulus Sampling Theory*, San Francisco, Holden-Day, 1967.)

The study of discrimination learning has become increasingly emphasized, leading to the multiple process theories of E. Lovejoy (*Attention in Discrimination Learning*, San Francisco, Holden-Day, 1968) and of D. Zeaman and B.J. House ("The Role of Attention in Retardate Discrimination Learning," in H.R. Ellis, ed., *Handbook of Mental Deficiency*, New York, McGraw-Hill, 1969), which have been applied to the study of learning in the mentally retarded; to developmental approaches to discrimination learning involving mediation (see Estes, 1970, *op. cit.*); and to the notions of "learning set" and "learning to learn" arising from the work of H.F. Harlow ("The Formation of Learning Sets," *Psychological Review*, Vol. 56, 1949). The work of D.E. Broadbent (*Perception and Communication*, Oxford, Pergamon Press, 1958) and T. Trabasso and G.H. Bower (*Attention in Learning*, New York, Wiley, 1968) has attempted to bridge the gap between the former emphasis on "attention" and other aspects of discrimination learning.

The increasingly active research on verbal learning and memory has yielded numerous findings concerning probability learning and probability matching (W.K. Estes, "Probability Learning," in W.A. Melton, ed., *Categories of Human Learning*, New York, Wiley, 1964), the functions of awareness, task orientation, and information in determining the effectiveness of rewards and punishments (J. Nuttin and A.G. Greenwald, *Reward and Punishment in Human Learning*, New York, Academic Press, 1968), and the distinction between long and short term memory. That distinction is found in a number of models for memory that are currently influential. (See D.A. Norman, *Models for Human Memory*, New York Academic Press, 1970.) Increasing attention is being given to organizational aspects of learning, particularly with reference to free verbal recall. (See, for example, W. Kintsch, *Learning, Memory, and Conceptual Processes*, New York, Wiley, 1970.)

Major lines of possible practical application of the results of learning theories include the study of problems of mental retardation (Estes, 1970, *op. cit.*); computer assisted education (R.C. Atkinson, "Computerized Education and the Learning Processes," *American Psychologist*, Vol. 23, 1968), and psychotherapy (Dollard and Miller, 1950, *op. cit.*).

Motivation and Emotion

The early preoccupation with basic emotions (fear, rage, elation, etc.) construed as unitary entities, and with such questions as the ability of people to identify emotions from facial expressions, has given way to research on the neurophysiological bases of emotional behavior, the tracing of the development of those patterns in childhood, and in-depth experimental analyses of selected examples.

The beginnings of a useful description of the relationship between emotions and bodily states was expressed in the classic James-Lange theory of emotion. (See R.S. Woodworth, *Experimental Psychology*, New York, Holt, 1938.) Studies of physiological mechanisms have uncovered the importance of the thalamus as a basis of primitive patterns of emotional behavior (R.A. McCleary and R.Y. Moore, *Subcortical Mechanisms of Behavior*, New York, Basic Books, 1965) and, more recently, have led to the notion of *reticular activation* and the more general notion of *arousal*, both of which are given considerable attention in contemporary work.

Experimental work conducted largely within the framework of Hull's behavior theory has concerned the importance of fear as an "acquired drive" in animal learning and the relations between "manifest anxiety" and learning and performance in human beings. (E.g., Spence, 1956, *op. cit.*) The developmentally-oriented study of emotion is exemplified in J.W.M. Whiting and I. L. Child, *Child Training and Personality: A Cross-Cultural Study* (New Haven, Yale University Press, 1953).

The first steps in quantifying aspects of motivation were achieved in C.J. Warden's development of the obstruction method for measuring animal drives (*Animal Motivation: Experimental Studies on the Albino Rat*, New York, Columbia University Press, 1931); later more developed techniques were worked out within the context of operant conditioning (Skinner, 1938, *op. cit.*) Using behavioral studies, extirpation methods, and direct electrical stimulation of the brain, psychologists have studied the subcortical brain processes involved in the basic drives. (McCleary and Moore, *op. cit.*; Grossman, *op. cit.*) Combinations of behavioral and physiological procedures have been used to study problems such as the motivational control of obesity. (E.g., Schacter, *op. cit.*) Combinations of experimental and psychological scaling methods have generated a substantial number of reports on achievement motives in both children and adults. (See J.W. Atkinson, *An Introduction to Motivation*, Princeton, Van Nostrand, 1964.)

Cognition and Thought

The development of the more complex intellectual processes in human beings was for a long time dominated almost exclusively by the work of Jean Piaget. (See, for example, his *Language and Thought of the Child*, New York, Harcourt, 1926.) More recently such work has been supplemented by other work in which general learning and behavior theory are emphasized (Estes, 1970, *op. cit.*) and in which combinations of experimental and ethnographic methods are used (Cole, Gay, Glick, and Sharp, *op. cit.*). At the adult level much recent inquiry has been directed toward investigating the role of strategies and rules in thinking and problem solving. (J.S. Bruner, J.J. Goodnow, and J.A. Austin, *A Study of Thinking*, New York, Wiley, 1956; F. Restle, *The Psychology of Judgment and Choice*, New York, Wiley, 1961.)

Experimental work in conjunction with the application of mathematical models has been used in an attempt to bridge the gap between learning processes having to do with discrimination and generalization and the more complex processes. (See, for example, R.C. Atkinson, G.H. Bower, and E.J. Crothers, *Introduction to Mathematical Learning Theory*, New York, Wiley, 1965; Trabasso and Bower, *op. cit.*) Recently conjectures derived from linguistics and computer information processing have also been used. (Noam Chomsky, "Three Models for the Description of Language," in Luce, Bush, and Galanter, 1965, *op. cit.*; W.R. Reitman, *Cognition and Thought*, New York, Wiley, 1965.)

Personality

The study of personality deals with the relatively general traits or dispositions that characterize an individual's responses to a variety of situations. Personality psychologists have often used interdisciplinary methods, drawing upon anthropology, sociology, and psychoanalysis in addition to experimental and developmental psychology. In its earlier phases the psychology of personality was rather sharply subdivided into the study of modes or mechanisms of adjustment growing out of psychoanalytic notions (for example sublimation, displacement, and projection) on the one hand, and, on the other, the investigation of individual differences in temperament, traits, and values.

The first branch led to studies of the processes of acculturation and socialization of the individual. (See, for example, I.G. Sarason, ed., *Contemporary Research in Personality*, Princeton, Van Nostrand, 1962, and C.S. Hall and G. Lindzey, *Theories of Personality*, New York, Wiley, 1957.) The second branch has proliferated into several distinct ways of attempting to measure personality variables. One line of development is represented by the Minnesota Multiphasic Personality Inventory in which the items comprising various scales are selected solely on the basis of their ability to differentiate groups of people falling in different psychiatric categories (S.R. Hathaway and J.C. McKinley, *The Minnesota Multiphasic Personality Inventory: Manual*, rev. ed., New York, Psychological Corporation, 1951; W.G. Dahlstrom and G.S. Welsh, *An MMPI Handbook: A Guide to Use in Clinical Practice and Research*, University of Minnesota Press, 1960). Efforts toward extracting major dimensions of personality from the results of tests and questionnaires depend primarily on the methods of factor analysis. (See, for example, R.B. Cattell, *Personality: A Systematic Theoretical and Factual Study*, New York, McGraw-Hill, 1950; J.P. Guilford and W.S. Zimmermann, "Fourteen Dimensions of Temperaments," *Psychological Monographs*, Vol. 70, 1956.)

More closely related to some psychoanalytic theories are projective tests in which the individual is presented with relatively unstructured material, for example ink blots, responses to which are assumed to reflect personality characteristics. (E.g., H. Rorschach, *Psychodiagnostics: A Diagnostic Test Based on Perception*, 3rd ed., New York, Grune, 1942; H.A. Murray, *The Thematic Apperception Test Manual*, Cambridge, Harvard University Press, 1943.) A relatively new aspect of personality research deals with the determinants of "creativity," utilizing a combination of experimental, case history, and measurement techniques. (See J.P. Guilford, "Some Theoretical Views of Creativity," in H. Helson and W. Bevan, *Contemporary Approaches to Psychology*, New York, Van Nostrand, 1967.)

Problems of the assessment and organization of "mental" abilities (psychometrics) have traditionally been treated separately from the psychology of personality. Principal landmarks are the initial development of individual intelligence testing by Binet in the early 1900's and the revision and standardization of the scale by Lewis M. Terman. (*Op. cit.*; see also Q. McNemar, *The Revision of the Stanford-Binet Scale*, Boston, Houghton Mifflin, 1942.) The same general procedures were extended to the development of a widely used adult intelligence scale by D. Wechsler. (*The Measurement of Adult Intelligence*, Baltimore, Williams and Wilkins, 1939.)

Group intelligence testing was initiated on a large scale with the U.S. Army's Alpha and Beta tests during World War I, and the group technique was subsequently expanded to provide for mass evaluation of aptitude and achievement in school children. The two most distinctive views on the organization of intellectual abilities have been the notion of a general intelligence ("g" factor), developed by C.E. Spearman (*The Abilities of Man: Their Nature and Measurement*, London, Macmillan, 1927), and the multiple factor point of view (L.L. Thurstone, "Primary Mental Abilities," *Psychological Monographs*, No. 1, 1938).

Many of the personality and psychometric tests have been severely criticized, both as to details and more generally, and there is a continuing effort to assess and assure the reliability and validity of such tests. (See Section on Contemporary Controversy.)

Social Psychology

Considerable effort has gone into the development of sampling and survey methods for the assessment of opinions and attitudes. (See L. Festinger and D. Katz, *Research Methods in the Behavioral Sciences*, New York, Dryden, 1953.) A byproduct of practical importance is the development of public opinion polls widely used in politics and elsewhere. Extensive results concerning the modification of attitudes and opinions were reported in the series of studies by Hovland and associates. (C.I. Hovland, I.L. Janis, and H.H. Kelley, *Communication and Persuasion: Psychological Studies of Opinion Change*, Yale University Press, 1953.)

Studies of the ways an individual maintains a serviceable, self-consistent set of beliefs in social situations have been made. (See L. Festinger, *A Theory of Cognitive Dissonance*, Evanston, Row Petersen, 1957.) Attempts have been made to develop mathematical transformations that will facilitate prediction of small group interactions. (E.g., J. Criswell, H. Solomon, and P. Suppes, *Mathe-*

matical Methods in Small Group Processes, Stanford, Stanford University Press, 1962.) In some instances experimental studies with animals have yielded apparently useful results for human social behavior, particularly analyses of the effects of early social deprivation upon the development of affectional systems. (H. Harlow, "The Nature of Love," *American Psychologist*, Vol. 13, 1958.)

Physiological Psychology

Developments in biochemistry, particularly the "breaking" of the genetic code and the discovery of DNA and RNA, have led to intense activity directed toward investigating the biological basis of memory by neurophysiological methods, but without much progress to date in terms of the development of warranted assertions. With respect to receptor processes and basic drive mechanisms, on the other hand, substantial progress has been achieved in describing specific neurophysiological and biochemical mechanisms. Investigations of the effects of hormones on behavior have contributed materially to the understanding of developmental processes and sexual behavior. (E.g., Ford and F.A. Beach, ***Patterns of Sexual Behavior***, New York, Harper, 1951.) In the rapidly expanding area of psychopharmacology, workers have developed such practical results as tranquilizers used to control neurotic and psychotic symptoms, and have attempted to develop useful accounts of psychophysiological processes. (W. Himwich and J.P. Schade, *Horizons in Neuro-Psychopharmacology*, New York, Elsevier, 1965.)

5. CONTEMPORARY CONTROVERSY

Although the earlier controversies relating to the development of schools or systems of psychology have largely been abandoned, some of the controversies of earlier days persist, usually in modified form.

One can still find numerous instances of the long-standing controversies between advocates of "hard" and "soft" methodologies, the former associated with quantitative and laboratory-oriented procedures, and the latter associated with more intuitive and field-oriented procedures. Some of the most severe criticisms have been made of widely used projective tests. H.J. Eysenck, for example, says that in the case of Rorschach tests, "detailed investigation revealed all dross and no gold." (*Handbook of Abnormal Psychology: An Experimental Approach*, New York, Basic Books, 1961, p. xii.)

Eysenck comments on his study of texts on psychiatry, abnormal, and clinical psychology as follows:

> "The perusal of some fifty of these left me in a state of profound depression, as none of them contained any evidence of properly planned or executed experimental investigations, or even the realization of the necessity for such. Nor did I find that concise and consistent framing of theories and hypotheses which usually precedes experimental investigation; all was speculation and surmise, laced with references to 'clinical experience.'" (*Ibid.*, p. xiv.)

In general, he believes psychologists have wanted to run before they can walk:

> "In its present humble state, psychology can at best support on a factual basis certain low-order generalizations; to go beyond these is to court disaster. Such generalizations are the building stones for all future advance; hence the importance of deriving them from the facts in a proper quantified manner." (*Ibid.*, p. xiii.)

The issue of behavior vs. conscious experience as the basic subject matter of psychology for some time seemed to have been largely resolved in favor of a "methodological behaviorism" in which "mental" events and processes were treated as behavior or inferences from observed behavior, and introspection was taken simply as one technique of observation that had no privileged status. However, the recent upsurge of existentialist, phenomenological, and humanistic psychology has tended to reinstate subjectivist procedures of inquiry. Such writers often link the methodological issue concerning consciousness to the alleged behaviorist denigration of human autonomy, freedom, dignity, etc.

In what probably is his most controversial book, B.F. Skinner has discussed both the methodological issues and the larger issues concerning what type of organism the human is. (*Beyond Freedom and Dignity*, New York, Knopf, 1971.) His book represents a sharp attack on subjectivism and mentalism. He argues that the behavioral sciences are a century behind the physical and biological sciences, primarily because of the persistence of prescientific ways of talking about human behavior. The attribution of behavior to "states of mind" such as purposes, intentions, and aims should be replaced by a direct investigation of the relations between behavior and environment.

Skinner is especially critical of the conventional notions of human freedom and autonomy, and urges that inquirers study the observed, the natural, and the manipulable, rather than the inferred, the miraculous, and the inaccessible. He emphasizes the control of the environment so that behavior likely to be punished will occur infrequently or not at all; more control, rather than less (as advocated by many critics of behaviorism), is what is needed.

The recent emphasis by some psychologists on freedom, autonomy, dignity, etc., and Skinner's counterattack, suggests that the whole set of controversies involved will again become a prominent part of psychological literature.

An issue that has generated some controversy, but perhaps less than its importance warrants, concerns the locus of behavior. The prevailing view is what we called *interactional* in Chapter 1: the individual is viewed as existing separately from his environment and as interacting with it. In contrast, other psychologists argue that behavior literally cannot be kept within the boundaries of the skin and that severing the individual from his environment seriously handicaps inquiry. The transactional procedures emphasize the entire organismic-environmental process in a field of activity. J.R. Kantor has long maintained a similar view; rather than the name "transaction," he uses "interbehavior." (*The Logic of Modern Science*, Bloomington, Principia Press, 1953, p. 262.)

Many of the controversial issues of a few decades ago no longer command much interest or attention, not so much because one side or the other "won," but because the issues became obsolete in the light of advances in inquiry or were fragmented into a number of more technical issues. Polemics concerning the relative merits of behaviorism and introspectionism have become considerably muted, but related differences in orientation continue to be visible, for example in behavioral vs. linguistic approaches to the acquisition of language and linguistic skills. (See T.R. Dixon and D.L. Horton, *Verbal Behavior and General Behavior Theory*, Englewood Cliffs, Prentice-Hall, 1968.)

So-called parapsychology, associated with J.B. Rhine and his Duke University colleagues, produces many claims and counterclaims. Not only the interpretations of the findings of the parapsychologists, but also the findings themselves, have been critically received. Although some prominent psychologists accept the occurrence of parapsychological phenomena, most contemporary American psychologists seem highly skeptical. (For examples of the controversy, see George R. Price, "Science and the Supernatural," *Science*, Vol. 122, 1955, and R.A. McConnell, "ESP and Credibility in Science," *American Psychologist*, Vol. 24, 1969.)

As in many other fields, recently some psychologists have argued that the dominant methods, problems, and findings of psychologists too often lack relevance, or are tied to the socioeconomic establishment, or are racist, or are anti-woman, etc. Many pages of articles, comments, and reports in recent issues of *American Psychologist* are devoted to such matters. Whether these controversies will increase or wane in the next few years is difficult to predict, but at the moment they are attracting considerable attention.

6. TERMINOLOGICAL PROBLEMS

Compared to workers in many other behavioral fields, psychologists have given much attention to the technical language they use. The influence of logical positivists such as Rudolph Carnap has been strong. (See, for example, Estes *et. al.*, 1955, *op. cit.*). Concerted efforts have been made to avoid mentalistic and nonbehavioral uses of words. (See William S. Verplanck, *A Glossary of Some Terms Used in the Objective Science of Behavior*, Washington, D.C., American Psychological Association, 1957.)

Difficulties arise, however. Certain names carried over from older mentalistic frameworks tend to retain some of their earlier connotations. For example, frequent use of "cognition," "cognitive," etc., is made, apparently to refer to some sort of knowing behavior, but it is frequently difficult to ascertain what is being named. Verplanck, referring to himself, says: "...the writer remains ignorant of what a *cognition* is. So far as he knows, he has never had one, and no one has ever been able to correct him on this, or tell him how to have one, or how to recognize it if he did." (*Ibid.*, p. ii.)

Perhaps the worst difficulties occur with "concept," which is very widely used by psychologists, including Verplanck. As Dewey and Bentley comment, often "concept" is "a word that is all things to all sentences." (Dewey and Bentley, *op. cit.*, p. 21.) Verplanck's own statement, intended to make "concept" usable, equates "concept" to:

> "...any response, verbal or motor, that is under the discriminative control of a broad class of environmental objects or events; the members of the class may differ from one another in all respects other than a single quantifiable property. Most concepts are statements that refer to the common property: 'blue,' 'square,' 'velocity,' 'beauty,' 'length.' Pseudoconcepts may depend on a number of partially overlapping classes of events that do not share an objective common property: 'honesty,' 'virtue,' 'rigidity.' " (Verplanck, *op. cit.*, p 7.)

Frequently concepts are viewed as the constituents of a statement, but here Verplanck says that most concepts are statements. If "honesty" is a pseudoconcept because the members of its class lack an "objective common property," how can "beauty" be a proper concept? If "velocity" is a concept, why is "rigidity" only a pseudoconcept? And how does Verplanck's definition fit with some of his own statements about concepts, such as: "Some terms are used in two ways: empirically, as a label for a class of behavior, and theoretically, as the name of a concept relating that behavior to other classes of events"? (*Ibid.*, p. ii.) One suspects that Verplanck's comments about "cognition" apply equally to "concept."

Other names used inconsistently relate to the issues discussed in Chapter 1 concerning *interaction* and *transaction*. For example, even those who take the subject matter of psychological inquiry as behavior often disagree in important ways about what "behavior" designates. Verplanck takes "behavior" as applying to "the whole complex of observable, recordable, or measurable activities of a living animal," and as including skeletal and muscle movements, glandular secretions, body chemistry changes, the making of sounds, etc. (*Ibid.*, p. 2.) Others focus on "the organism's external activity which interacts with the environment, as distinguished from the internal processes of growth and maintenance." (R.S. Woodworth, *Dynamics of Behavior*, New York, Holt, 1958, pp. 21-22.) Yet others, as noted, emphasize the entire organismic-environmental process and do not restrict the locus of behavior to within the organism.

Conventional use of stimulus-response notions is interactional and assumes a detachability or severability of stimulus from response as well as their ability to interact. Such procedures frequently lead to incoherence and a literal inability to maintain the separates assumed in the inquiry. To illustrate, one discussion in which a "narrow definition" of "stimulus" as a "unit of input" is adopted (e.g., "some definable unit of light falling on and exciting the receptors in the retina of the eye") gets into difficulty because there seems to be no way to hold as separate and to differentiate adequately in inquiry the "environmental object," the "sensory event," and "the unit as experienced by the observer." (Bernard Berelson and Gary A. Steiner, *Human Behavior: An Inventory of Scientific Findings*, New York, Harcourt, Brace & World, 1964, pp. 87-88.)

Verplanck notes five different uses of "stimulus" and says:

> "Fortunately for the intellectual comfort of the reader (but for nothing else), in most cases the ambiguity of this term does not reveal itself, since most students of behavior have not shown any great interest in treating the problem of stimulation in great experimental detail." (Verplanck, *op. cit.*, p. 33.)

He differentiates: 1) "a physical event impinging on the receptors of an animal," and says that usage "is perhaps always incorrect"; 2) "a physical event impinging on the receptors of an animal and capable of exciting those receptors" 3) "a specified part, or change in a part, of the environment correlated in an orderly manner with the occurrence of a specified response" (Skinner's usage); 4) "an event within the animal hypothesized to account for certain complex behavior"; and 5) "loosely used as synonymous with stimulus object (an object which produces stimuli) and with stimulus event (an event which produces stimuli)." (*Ibid.*, p. 33.)

Other names supposedly useful in the development of descriptions and warranted assertions have led to difficulties and controversies. There has been considerable discussion of "intervening variable" and "hypothetical construct." (See Paul E. Meehl and Kenneth MacCorquodale, "On a Distinction Between Hypothetical Constructs and Intervening Variables," *Psychological Review*, Vol. 55, 1948.) Tolman originally proposed the intervening variable as a construct operationally defined by specific sets of antecedent and consequent (i.e., stimulus and response) variables. In time, "intervening variable" was used to refer to many other things and sometimes was used as a magical device to avoid embarrassing problems. Meehl and MacCorquodale suggest that "intervening variable" be reserved for Tolman's original usage and that "hypothetical construct" be used for situations containing more that a stimulus-response relationship.

Here we see the interrelations of some of the terminological problems, including "stimulus," "response," and "operational definition." After operationism was advocated by Percy Bridgman (*The Logic of Modern Physics*, New York, Macmillan, 1927), many psychologists adopted some variant as a useful tool in psychological inquiry. "Operationism" is a name used in different ways. Verplanck, for example, although he emphasizes "objective" and "behavioral" procedures of inquiry, gives a rather traditional account: "the general point of view toward the data and concepts of natural science which holds that the concepts of a science are defined by the experimental operations involved in investigation and measurement." (Verplanck, *op. cit.*, p. 23.)

Skinner, on the other hand, emphasizes the reports a scientist makes about his methods:

> "Operationism may be defined as the practice of talking about (1) one's observations, (2) the manipulative and calculational procedures involved in making them, (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) *nothing else.*" ("The Operational Analysis of Psychological Terms," *Psychological Review*, Vol. 52, 1945, p. 270.)

Some commentary on "theory" seems appropriate. Sometimes psychologists use the word to designate highly warranted descriptions (as in "theory of evolution"); sometimes to designate an inclusive, but not yet confirmed, set of conjectures or hypotheses in some area ("learning theory"); sometimes to designate an internally consistent set of assumptions and logical derivations from those assumptions ("game theory"); sometimes to designate speculative systems not likely to be confirmed ("Freudian theory"); sometimes as equivalent to "conjecture" as we used that name in Chapter 1.

Finally, in the clinical and social areas a number of names are often used unclearly or ambiguously; for example, "personality," "normal," "integration," and "mental health."

7. COMMENT AND EVALUATION

A major contribution of psychologists to the behavioral sciences has been the development of widely useful research methods. Techniques of measurement and scaling developed by psychologists are basic tools in many of the behavioral sciences and are widely used in problems of selection and evaluation in industry, education, etc. The adaptation to behavioral inquiry of methods of analysis of variance, multivariate data analysis, and the principles of experimental design, has been highly developed.

Many well-supported findings have resulted from the inquiries of psychologists, but those findings often are not integrated in a way useful for the solution of large scale human problems. Some warranted assertions have been developed that are as quantitatively precise and as useful in making predictions as are many warranted assertions developed by physical scientists. As noted, however, at present there is a renewed tendency for some psychologists to advocate the use of nonscientific procedures. The rapid expansion of both academic and applied psychology in recent years has had its drawbacks—chiefly that attempted applications have outrun well-supported findings. This trend has been especially conspicuous in clinical psychology, which during and immediately following World War II was subject to a demand for "solutions" for which there was no adequate scientific groundwork.

Probably the best supported findings to date have occurred in the physiological areas of psychology and with animal subjects. Much of the experimental work of behaviorists such as Skinner seems comparable in scientific rigor to the work done in the physical science fields. However, inquiries in the more distinctive human areas (what we called sign-behavior in Chapter 1), seems far less advanced. Some psychologists hope that eventually sign-behavior can be described adequately using the characteristic procedures of inquiry found in the physical and physiological fields, but we suggest that such hopes are not likely to be realized.

Perhaps the recent upsurge of existential and humanistic psychologies is to a considerable extent the result of the failure of behaviorists and others to solve the problems of sign-behavior using only physical and physiological procedures of inquiry. The relative failure to solve such distinctively human problems suggests strongly the importance of the work of Kantor, Cameron, and some of the recent transactional psychologists, who do not sever the organism from the environment, mind from body, the individual from the social, etc. In short, quite possibly further "substantive" progress in sign-behavior will not occur until improvements are made in the procedures of inquiry.

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BRITISH JOURNAL OF PSYCHOLOGY
CANADIAN JOURNAL OF PSYCHOLOGY
CHILD DEVELOPMENT
COGNITIVE PSYCHOLOGY
COMPARATIVE GROUP STUDIES
CONTEMPORARY PSYCHOLOGY
DEVELOPMENTAL PSYCHOLOGY
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JOURNAL OF EDUCATIONAL PSYCHOLOGY
JOURNAL OF EXPERIMENTAL PSYCHOLOGY
JOURNAL OF EXPERIMENTAL CHILD PSYCHOLOGY
JOURNAL OF HUMANISTIC PSYCHOLOGY
JOURNAL OF MATHEMATICAL PSYCHOLOGY
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SOCIOMETRY

III.

ANTHROPOLOGY*

1. WORKING DESCRIPTION OF THE FIELD

ANTHROPOLOGISTS inquire into the behavioral similarities and differences of human cultural groupings, from earliest man to the present. The major subfields are: the study of the organization of cultures and the development, distribution, and functions of customs, techniques, and other culture traits (cultural anthropology); the study of the evolution of human physical characteristics in their cultural settings (physical anthropology); the study of past cultures through the excavation and investigation of material remains (archaeology); and the study of human languages in their cultural settings (linguistics). The inquiries of anthropologists overlap considerably with work done by biological and other behavioral scientists, as well as with work done in the humanities.

2. OTHER DESCRIPTIONS OF THE FIELD

Clyde Kluckhohn's statement of nearly twenty years ago is representative of the breadth claimed for anthropological inquiry and the main areas in which research is conducted:

> "Anthropology is the study of the similarities and differences, both biological and behavioral, among the peoples of the world from the dawn of human history to the present day. Anthropology excavates and analyzes the remains of past civilizations (archaeology); describes the evolution and present biological characteristics of our species (physical anthropology); traces the development and spread of customs and technologies over the face of the earth, showing how these forms, arts, faiths and tools satisfy the psychological needs of individuals and keep societies together (cultural anthropology); defines the varieties of human speech and the relationships among the tongues of men (linguistics)." ("Anthropology," in James Newman, ed., *What Is Science?*, New York, Simon and Schuster, 1955, p. 319.)

Many of the descriptions found in the literature claim an extremely broad subject matter for anthropology. In the 1930's Franz Boas maintained that anthropological subject matter "includes all the phenomena of the social life of man without limitation of time and space." (*Encyclopaedia of the Social Sciences*, Vol. II, p. 73.) In later years, anthropology was viewed as "the science of man" (Ralph Linton, ed., *The Science of Man in the World Crisis*, New York, Columbia University Press, 1945); as "that discipline which claims an interest in 'man and his works' at all times and in all places" (Gail Kelly, "Anthropology," in Bert F. Hoselitz, ed., *A Reader's Guide to the Social Sciences*, Glencoe, Free Press, 1959, p. 189); and as the field dealing with "the origin, development, and nature of man and his culture" (Allen H. Smith and John L. Fischer, eds., *Anthropology*, The Behavioral and Social Sciences Survey, Englewood Cliffs, Prentice-Hall, 1970, p. 5).

As will be discussed more thoroughly later, the broad range of concerns claimed for anthropology has had as one result that widely varying procedures of inquiry have been used—some scientific and some characteristic of scholarship in the humanities. As Cora Dubois says:

> "American anthropologists consider that their subject properly encompasses the biologic, psychologic, social, and cultural aspects of man. Nothing human is foreign to them. They have embraced enthusiastically and immodestly the literal meaning of the word anthropology—the science of man. Not satisfied with the *science* of man, they honor many in their profession who are avowed humanists." ("Anthropology: Its Present Interests," in Bernard Berelson, ed., *The Behavioral Sciences Today*, New York, Basic Books, 1963, p. 26.)

And Kelly maintains that:

> "If it [anthropology] cannot demonstrate that it does, in fact, successfully encompass the study of man in all aspects of his existence, anthropology can at least claim that it offers a greater variety of knowledge and approaches to this study than does any other field. (Kelly, *op. cit.*, p. 189.)

In practice, anthropological inquiries have tended to be differentiated from inquiries in other behavioral fields in four ways. Anthropologists often emphasize: 1) comparative inquiries (present men compared to past men; cross-cultural studies, etc.); 2) relatively small groups, often nonliterate, which are not as complex as some larger groups, tend to be isolated in some respects, and tend not to be studied by workers in other behavioral fields; 3) field work in which the anthropologist functions as a "participant-observer"; and 4) the customs, beliefs, rituals, technologies, etc., characteristic of the group as a whole. However, workers in other fields also have inquired into the matters just mentioned; and some work done by anthropologists is not comparative, much recent anthropological work has been done on literate, relatively complex cultural groups, inquiry into past cultures cannot involve the participant-observer method, and a "holistic" emphasis is not always found in anthropological inquiry.

Anthropological inquiry, then, is not sharply differentiated from work in other behavioral fields either in subject matter investigated or in methods used. And the grouping of the four major subfields mentioned within anthropology in part is accidental:

> "This grouping of somewhat disparate subjects under the banner of anthropology is in part a response to a special historical situation: the rapid acculturation and disintegration of many American Indian tribes during the formative period of American anthropology before and after the turn of the century. In their field research the early anthropologists encountered small neglected groups of people, apparently about to lose their identity, who were the last representatives of formerly flourishing native societies. The investigators felt an

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obligation to science to record all possible information about these vanishing groups: a complete naturalistic description of physique, language, customs, traditions, and even a little of the local archaeology if possible." (Smith and Fischer, *op. cit.*, p. 22.)

3. METHODS AND TYPES OF INQUIRY

As was noted earlier, a wide variety of both general procedures and specific methods of inquiry are found in use among anthropologists, including psychoanalytically-oriented inquiry (from the extreme version found in Géza Róheim's *Psychoanalysis and Anthropology*, New York, International Universities Press, 1950, to the milder version found in J.W.M. Whiting and Irvin L. Child's *Child Training and Personality Development*, New Haven, Yale University Press, 1953), formal and mathematical models (Hans Hoffman, "Mathematical Anthropology," in Bernard J. Siegel, ed., *Biennial Review of Anthropology-1969*, Stanford, Stanford University Press, 1970; Paul Kay, ed., *Explorations in Mathematical Anthropology*, Cambridge, MIT Press, 1971; Ira R. Buchler and Henry A. Selby, *Kinship and Social Organization*, New York, Macmillan, 1968), game theory applications (see articles by Walter Goldschmidt and Richard F. Salisbury in Ira R. Buchler and H.G. Nutini, eds., *Game Theory in the Behavioral Sciences*, Pittsburgh, University of Pittsburgh Press, 1969), and a host of other literary and purported scientific methods. In this Section we will consider first some of the general procedures of inquiry used by anthropologists, and then review some of the more specific methods used in the four major subfields.

General Procedures of Inquiry

In terms of our discussion in Chapter 1, anthropological inquiry tends to be partially transactional, in that typically the focus is on a full process in a field having many mutually connected aspects and phases. However, the "holistic" procedures used by some anthropologists are also partially self-actional and interactional; warranted assertions are sometimes freely mixed with elaborate speculation, apparently in order to "fill out the picture." Fairly often the objective seems to be the development of a plausible, internally consistent "reconstruction" of a culture, a developmental stage in human evolution, etc., rather than achieving warranted assertions.

There appears to be less interest among anthropologists than among inquirers in many other behavioral fields in both the usefulness of applying modern scientific procedures and in what those procedures are. The confusion is great; sometimes those criticized as following unscientific procedures claim that they want to model anthropology after the physical sciences (e.g., Claude Lévi-Strauss); quite often scientific inquiry is construed as involving the imaginative elaboration of conjectures in advance of testing (see Ino Rossi, "Reply to Nutini's 'The Ideological Bases of Lévi-Strauss's Structuralism,'" *American Anthropologist*, Vol. 74, 1972); and there is disagreement even about such topics as what an experiment is (see Hollis D. Paul's review in *Behavioral Science*, Vol. 17, 1972, of Michael Cole, John Gay, Joseph A. Glick, and Donald W. Sharp, *The Cultural Context of Learning and Thinking: An Exploration in Experimental Anthropology*, New York, Basic Books, 1971).

To give only one further example of what we regard as great confusion, in a recent publication Paul Kay has defended the objective of studying the human "mind." He goes on to say:

> "I am sorry if that word offends anyone, but to pretend that human beings do not have minds because a mind has never been seen or touched would require that we reject virtually all highly abstract and therefore useful scientific concepts, including, for example, gravity, relativity and probability." ("Some Theoretical Implications of Ethnographic Semantics," in Ann Fischer, ed., *Current Directions in Anthropology*, Washington, D.C., American Anthropological Association, 1970, p. 30.)

Not only is the "therefore" strangely placed in his assertion, but his argument, as stated, is a non-sequitur, as will be noted if *luminiferous ether* is substituted for *human mind*. Rejecting one "abstraction" as useless certainly does not imply that all "abstractions" are useless. In addition, Kay's statement seems to require considerable clarification with reference to what *observation* is; surely a restriction to what can be "seen or touched" by the unaided senses (as is apparently his view) is not required.

An autobiographical statement by Pertti J. Pelto suggests that often the formal training of anthropologists puts little emphasis on scientific method:

> "When I embarked on my first major anthropological research venture. . .I had had no formal training in the logic and structure of social sciences research. Many of my peers have described a similar lack of methodological preparedness in the years of their doctoral candidacy. Our generation of anthropologists, trained in the 1950s, learned the descriptive and theoretical contributions of our predecessors, but not how these anthropological contributions were achieved. We were not unconcerned about how field research is carried out–in fact we were almost frantic to find out–but we were assured by our teachers that we could learn the mysteries of field work only through personal immersion in the practically undescribable but romantically alluring complexities of the field. Much of the lore about field research that we picked up informally in our graduate-student days was concerned with the gentle arts of rapport-building and role-playing in field situations. We were not so much concerned, nor were our mentors, with rules of evidence, questions of 'representativeness,' 'validity,' 'reliability,' and the many other related elements of scientific inquiry with which our friends in other social sciences seemed to be preoccupied." (*Anthropological Research: The Structure of Inquiry*, New York, Harper & Row, 1970, pp. xi-xii.)

Despite what has just been said, "natural science" procedures of inquiry have been strongly urged by many major figures in anthropology. A.L. Kroeber, for example, argued that "culture" is "wholly an evolutionary development within nature, and therefore to be investigated by the *methods of fundamental natural science*" (*Anthropology Today: An Encyclopedic Inventory*, Chicago, University of Chicago Press, 1953, p. xiv, italics added), and he believed that the anthropologists of his time generally followed "naturalistic" procedures:

> "What the past half-century has accomplished above all for anthropology [is the development of a common naturalistic procedure].... It insists on treating the customs and histories, the ideals and values, the societies and languages of man as being phenomena of nature to exactly the same degree as the biology of men, or for that matter of animals and men. This may seem a simple and trite program. Perhaps it is simple conceptually, but operationally it has been difficult and hard-won. How far are men and their activities actually treated as a part of nature in most economic and sociological study, in most history and philology? Hardly at all; in these fields human activities are consistently set apart from nature...." (*The Nature of Culture*, Chicago, University of Chicago Press, 1952, p. 143.) (Kroeber also tended to emphasize anthropology as a "historical science" and was suspicious of the social sciences of his day. See "The Place of Anthropology in Universities," *American Anthropologist*, Vol. 56, 1954.)

Leslie A. White has argued vigorously and polemically over a long period for scientific procedures of inquiry into human behavior, including symbolic behavior. (See especially his essays "Science is *Sciencing*" and "Mind is *Minding*," in his *The Science of Culture*, New York, Farrar, Straus, 1949.) George P. Murdock has urged that anthropologists become more scientific. In relation to sociologists, he says, anthropologists are:

> "...extraordinarily naive in scientific matters. Many frankly confess a humanistic rather than a scientific orientation, and not a few are openly anti-scientific. Among those who are actually engaged in research problems which can be classed as scientific, only a handful are adequately grounded in scientific method, and many of these are committed to one method and are skeptical of others. Those who, like Kluckhohn, are both genuinely sophisticated and broadly oriented are rare indeed. In sociology, men with a flaming zeal for science are not uncommon. The writer thinks instantaneously of such diverse figures as Keller, Lundberg, and Stouffer, but he has never encountered such a man in anthropology. The low status of scientific interest and awareness in anthropology is assuredly the most serious handicap to the full participation of this discipline in the integrated human science of the future." ("Sociology and Anthropology," in John Gillin, ed., *For a Science of Social Man*, New York, Macmillan, 1954, pp. 26-27.)

In archaeology, the so-called "new archaeologists" argue for explicitly scientific procedures of inquiry that will help "to integrate archeology with the mainstream of the social sciences." Considerable emphasis is put on the "hypothetical-deductive" method and the relevance of prehistoric data to the description of cultures. (See, for example, Patty Jo Watson, Steven A. LeBlanc, and Charles L. Redman, *Explanation in Archeology: An Explicitly Scientific Approach*, New York, Columbia University Press, 1971, and Lewis R. Binford, *An Archaeological Perspective*, New York, Seminar Press, 1972.) The "new archaeologists" have not lacked for critics, and have been accused of not being scientific enough—they are said to have relied on dubious generalizations from other fields, to have misused statistical techniques, to have reached unreliable conclusions on the basis of highly fragmentary data, etc. (For some such criticisms, see Leroy Johnson, Jr., "Problems in 'Avant-Garde' Archaeology," *American Anthropologist*, Vol. 74, 1972.)

Physical anthropologists have basically used, and continue to use, the general scientific procedures found in the biological sciences. Linguists for some time were strongly committed to scientific procedures in their descriptions of languages, to the extent that observers often placed linguistic inquiry as among the most advanced in the behavioral areas. (See, for example, the assessments by Kluckhohn, *op. cit.*, p. 346, and John B. Carroll, *The Study of Language*, Cambridge, Harvard University Press, 1959, p. 66.) More recently, even in anthropological linguistics, the Chomsky "revolution" has given rise to the adoption of mentalistic methods. (See the Chapter on Linguistics.)

As is also characteristic of many other behavioral inquirers, some recent anthropologists who advocate scientific procedures put great emphasis on theory-construction and the use of formal models elaborated far in advance of observation and measurement of changes. (See Chapter 1 for our criticisms of such premature "theorizing.") That issue aside, there appear to be two main points of view opposing scientific procedures: the "historical" and the "qualitative."

The strongest version of anthropology as culture history was developed by the school associated with Franz Boas. Boas opposed any simplistic determinism such as the older evolutionism and racial or environmental determinisms. His skepticism seems to have led some of his followers to emphasize "particularistic" descriptions and to avoid more general inquiries. Through the influence of Boas and others, the point of view sometimes called The American Historical School became established as nearly the "official" anthropology. His students, among them Clark Wissler and A.L. Kroeber, exerted a strong influence on the subsequent course of anthropological inquiry. The American Historical School regarded anthropology as primarily culture history, and was characterized by "a straightforward desire to know what happened in the past, especially in the ages before writing and in areas without writing." (C.W.M. Hart, "Cultural Anthropology and Sociology," in Howard Becker and Alvin Boskoff, *Modern Sociological Theory*, New York, Dryden, 1957.) Field work was strongly emphasized; workers were judged primarily on the basis of their field work, rather than on the general conclusions they attempted to develop from that work. As Hart observes:

> "Nothing better demonstrates the natural-history birthright of cultural anthropology than the fact that sheer description of a natural phenomenon, or of human behavior treated as a natural phenomenon, is still regarded as both a primary duty and a problem in itself...." (*Ibid.*, p. 538.)

Although the disputes between those who saw anthropology as scientific and those who saw it as "historical" have waned, traces of the historical view remain, and some, such as E.E. Evans-Pritchard, argue that anthropology should be historical, particularistic, and "idiographic." (For a general discussion of the alleged oppositon between science and history, see the Chapter on History.)

The "qualitative" view appears to have much in common with the so-called "qualitative methodologists" in sociology. At least for some problems, it is maintained, scientific procedures must be supplemented or replaced by subjective or introspective procedures. Oscar Lewis has described the situation of some years ago as follows:

> "On the one hand, there are those who would underscore the kinship of anthropology with the natural sciences, would stress the need for quantification, objective tests, experiments, and a general development and improvement of techniques which might lead to great precision and objectivity in the gathering, reporting, and interpreting of field data. On the other hand, there are those who, though not denying for a moment the kinship of anthropology with the sciences, believe that what needs to be stressed at this time is the kinship of anthropology with the humanities, and, accordingly, they would emphasize the need for insight, empathy, intuition, and the element of art." ("Controls and Experiments in Field Work," in *Anthropology Today*, p. 453.)

More recently, some anthropologists, although insisting on the scientific nature of their work, have emphasized the use of methods characteristic of some humanistic scholarship and have relied heavily on mentalistic assumptions. For example, practitioners of "ethnoscience" (sometimes called the "new ethnography," "ethnosemantics," "emic analysis," etc.) investigate the "meanings" through which a cultural group organizes "reality." Considerable attention is given to the beliefs, expectations, etc., shared within a culture that help to make the behavior of the members mutually intelligible. According to Paul Kay: "The system of meanings—the tacit theory of the world—lying behind a language and its usage is a natural phenomenon worthy of scientific study." (In Ann Fischer, *op. cit.*, p. 20.)

Such work is similar in certain respects to that done in sociology by the "symbolic interactionists" and the "ethnomethodologists." (See the Chapter on Sociology.) Erving Goffman's use of symbolic interactionist procedures has been influential among anthropologists studying inter-group relations. (Goffman, *Relations in Public*, New York, Basic Books, 1971.) There are also certain similarities to the work done by the "subjective behaviorists" in psychology, who make use of subjective methods and talk about the "mind." (George A. Miller, Eugene Galanter, and Karl R. Pribram, *Plans and the Structure of Behavior*, New York, Holt, Rinehart and Winston, 1960, p. 211.) Perhaps because of the difficulties an "outsider" faces in trying to understand another culture from the "inside," there also has been some attention given to presumed fundamental epistemological problems in anthropology. (See F.S.C. Northrup and Helen H. Livingston, eds., *Cross-Cultural Understanding: Epistemology in Anthropology*, New York, Harper & Row, 1964; Edmund Leach, *Rethinking Anthropology*, London, Athlone Press, 1961.)

Claude Lévi-Strauss' "structural anthropology" has been given much attention, and he has been described as "one of the vital intellectual figures of the contemporary world." (Smith and Fischer, *op. cit.*, p. 37.) In a series of books (among them, *Structural Anthropology*, New York, Basic Books, 1963; *The Savage Mind*, Chicago, University of Chicago Press, 1966; and *The Raw and the Cooked*, New York, Harper & Row, 1969), Lévi-Strauss has attempted to discover the elementary structures governing the way men marry, mythologize, construct their language, and understand their world. He regards myths as ways in which humans solve the problems facing them. He views *nature* and *culture* as two different ontological levels, and sees culture as a way of coding and organizing natural reality. (Hugo G. Nutini, "The Ideological Bases of Lévi-Strauss's Structuralism," *American Anthropologist*, Vol. 73, 1971.) In general, Lévi-Strauss argues, in a somewhat Kantian way, that man organizes reality through a binary mode in which dialectically related "opposites" (such as the raw and the cooked) are extremely important. Despite the frequent praise given Lévi-Strauss for his insights, elegance, and sophistication, many critics have been harsh. Robert T. Anderson, for example, says:

> "The prominence presently given to structuralism seems disproportionate to its achievements. It is an approach that purports to offer an integrated body of theory. In fact, it is hard to see where it has done much more than provide a way of looking at certain phenomena, including marriage alliances in some societies." ("Recent Trends in Ethnology: 1966-1970," in *The Annals*, Vol. 401, May, 1972.)

The emphasis on searching for deep structures underlying what we can observe more directly is found in many fields, and recently much excitement has been generated by such work. Noam Chomsky's transformational grammar, for example, concerns the transformational rules by which the "surface" structure of the grammars of specific languages are generated from allegedly "deep" innate structures. (See Jacques Ehrmann, ed., *Structuralism*, Garden City, Doubleday, 1970, for a discussion of many applications of structuralism.)

Specific Techniques Used

Physical Anthropology. Several conventionally differentiated fields, in addition to physical anthropology, inquire into human evolution and the structure and operations of the human body. The general focus of much recent work in physical anthropology has been on the complex interconnections of cultural and genetic factors in human biology, which involves many techniques of inquiry. As Frederick S. Hulse says:

> "Culture is seen as the unifying theme of our studies, whether we are observing the behavior of baboons, subjecting laboratory animals to environmental stress, operating an electrophoresis apparatus, or engaged in statistical calculations. The cultural nature of man is an aspect and a product of his cultural nature. Man in nature is not distinguishable from man in culture, for our nature is cultural." (*The Human Species: An Introduction to Physical Anthropology*, 2nd ed., New York, Random House, 1971, p. 485.)

Much attention has been given (by paleoanthropologists and others) to the "reconstruction" of the biocultural evolution of man, which involves the use of biological techniques of inquiry as well as physical science techniques for dating ancient remains. The older emphasis on the gross morphological characteristics of humans and on typologies for racial classification has been superseded to a considerable extent by a concern for the distribution of particular traits among human populations, including much work on the distribution of blood groups, hemoglobin variants, serum factors, etc. Human growth and development also is studied, particularly as related to environmental stress (extremes of climate, altitude, diet, etc.). Techniques of "naturalistic description" also are used, as when the locomotion, feeding habits, social behavior, and communication of primates are observed in order to help describe evolutionary changes.

Recently a technological revolution in physical anthropology has occurred, involving radiographs, reflection colorimeters, spectrophotometers, paper chromatography, starch-gel electrophoresis, etc., as replacements for many older techniques of measurement. (See Stanley M. Garn, "The Newer Physical Anthropology," *American Anthropologist*, Vol. 64, 1962.)

Archaeology. H.L. Movius, Jr., summarizes the major interests of archaeologists as follows:

> "(1). . .the establishment of a relative time scale by means of which the fossil remains of early man and his cultural relics may be placed in their proper sequence; (2). . .the study of developing technology and material culture, as well as the survival(s) of ancient tool-making habits or traditions; and (3). . .the reconstruction of the changing environmental (biogeographical or ecological) conditions. . . which confronted man." ("Old-World Paleolithic Archaeology," *Bulletin of the Geological Society of America*, Vol. LX, 1949, pp. 1448-1449.)

In such work, archaeologists have used scientific, historical, and humanistic techniques. As Gordon R. Willey says:

> "To begin, [the archaeologist] is a historian in the broader sense of that term. Like the historian, his studies are directed toward retelling the human past, and his study should be a dynamic and integrated one, not a series of static facts. He is a scientist, too, or strives to be one, in that archaeology is a part of anthropology, and he is interested in cause and effect as well as generalizations about human social behavior and the development of civilization. Finally—and this is in no way incompatible with his scientific interests—he is a humanist." ("Archaeology: The Snows of Yesteryear," in Lynn White, Jr., ed., *Frontiers of Knowledge in the Study of Man*, New York, Harper, 1956, p. 49.)

Physical science techniques are used in the exploration and evacuation of sites and the analysis of data. Carbon 14 dating, chemical and geological analyses of soils and rocks, and botanical techniques such as dendrochronology are used. Work also is done concerning the application of electronics and atomic physics to the exploration of sites from the surface. Computerized analyses of data and simulation of archaeological processes are also employed. Many other complex techniques are coming into prominence. (See Raymond E. Chaplin, *The Study of Animal Bones from Archaeological Sites*, New York, Seminar Press, 1971, and the review by Dexter Perkins, Jr., in *Science*, Vol. 176, June, 1972.)

The introduction of so many new techniques has involved new problems. C.C. Lamberg-Karlovsky lists "cultural ecology, systems theory, computer techniques for quantification, the many and varied techniques of physico-chemical analysis applied to archaeological materials, [and] the development of models to describe and explain the history of human adaptation" as among the techniques that have proliferated faster than "our ability to find their application or organization within archaeological research programs." ("Operations Problems in Archeology," in Ann Fischer, *op. cit.*, pp. 111-112.)

Anthropological Linguistics. Our main discussion of linguistic techniques of inquiry is contained in the Chapter on Linguistics; consequently only a few points will be made here. The field often is divided into *descriptive linguistics*, in which language systems or dialects are described at a given stage in their histories, and *historical linguistics*, in which the history of the changes, developments, and relationships among languages is investigated. The latter field has been more speculative than the former, and conjectures have been extended far beyond the available evidence. A wide variety of special techniques has been developed to describe the structure of languages, considering them primarily as *vehicles* of communication. What is communicated in a given language is only of indirect concern; the emphasis is on the characteristic "patternment" or ways in which the elements of a language are combined by native speakers. A language is often viewed as a particular system for coding what the language user wishes to say; the concern is with the code revealed in the utterances of the language user rather than with the content of the utterances.

Cultural Anthropology. Cultural anthropologists use a wide variety of specific techniques of inquiry, all of which probably are found in use in other fields. Perhaps the main difference is that cultural anthropologists have placed much more emphasis than do workers in other behavioral fields on intensive and protracted field work in cultures other than their own; field work that involves participation in the culture (the "participant-observer method"), interviewing, and the systematic collection of life histories, case studies, myths, and many other materials. (This is not, of course, to deny that anthropologists also sometimes study their own culture.) In analyzing and obtaining the field work data, the techniques, findings, and conjectures from many other fields—behavioral and literary—have been used.

4. RESULTS ACHIEVED

Surveying the general assessments of what has been achieved by anthropologists reveals an interesting alternation between highly favorable and highly negative judgments. To illustrate, since the time of Lewis Henry Morgan, kinship studies have occupied a central place in anthropological research, and often that work is assessed as a great accomplishment. (See Priscilla Reining, ed., *Kinship Studies in the Morgan Centennial Year*, Washington, D.C., Anthropological Society of Washington, 1972, and Meyer Fortes, *Kinship and the Social Order*, Chicago, Aldine, 1969.) On the other hand, J.A. Barnes concludes that only a "few scraps and odd bits" have been achieved so far in kinship studies. (*Three Styles in the Study of Kinship*, Berkeley, University of California Press, 1971, p. 265.) Much well-supported information has been developed about different peoples and cultures throughout the world, and yet Pelto notes that unfortunately anthropologists have "not succeeded in eliminating any except the more outlandishly improbable theoretical positions," and that most attempts to replicate studies are utter failures. (Pelto, *op. cit.*, p. 315).

In some areas of linguistics, highly useful results have been achieved. According to John B. Carroll:

> "It can be confidently stated that linguistic methodology is so well advanced today that if we took two fairly bright graduate students, trained them in linguistic analysis at two different universities, and sent them out to make descriptions of a new and unanalyzed language at two different times and using two different informants (assuming the informants spoke the same dialect), the resulting analyses would be highly similar, differing only in matters of emphasis and minor detail." (Carroll, *op. cit.*, p. 30.)

In other areas, far less firm results are found. According to Paul L. Garvin:

> "A look at the development of historical linguistics will show that there has been no scarcity of explanatory theories about linguistic history. Note, however, how most of these explanations, no matter how attractive they may have seemed at the time they were proposed, have since been relegated to oblivion." (*Method and Theory in Linguistics*, The Hague, Mouton, 1970, p. 11.)

In archaeology, as was noted earlier, the "new archaeologists" are highly critical of much prior work, but in turn themselves are criticized for taking speculations as warranted assertions. And similar differences of views can be found in the area of physical anthropology. (See Gabriel W. Lasker, "Physical Anthropology: The Search for General Processes and Principles," *American Anthropologist*, Vol. 72, 1970.)

Although some such differences of assessment can be found in all the behavioral fields, the differences seem greater in anthropology than in most other areas. A major reason, we suggest, is that anthropologists so often attempt "reconstructions" based on insufficiently warranted assertions. Many highly warranted "pieces" of the reconstruction may be available, but the conjectures often move far beyond the available evidence. Rather than the interweaving between conjectures and observation that we described in Chapter 1, elaborate "theorizing" based on some—but not enough—evidence often is undertaken. This is illustrated in the earlier quotation from Pelto, in which he complains about the inability to refute any but the most outlandish theories. In view of the number of plausible (to someone) conjectures that can be imagined about the connections among facts and assumed facts, to construe the task as refuting as many of the conjectures as possible seems "upside-down"; useful inquiry, we suggest, involves testing conjectures by repeated return to observation, rather than the development of elaborate conjectures that are logically consistent with scanty data.

In the following review of some of the representative results in the major subfields, we have not attempted to summarize all the work done by anthropologists.

Physical anthropologists have made progress in helping to trace the evolutionary development of humans. The proliferation of fossil discoveries during the past several decades has aided in the hypothetical reconstruction of the probable stages of evolution, and improved techniques of dating have been useful (for example, the development of potassium-argon dating suggests a time span of approximately 2-3 million years for the Pleistocene period). The fossil record suggests a relatively early development of a bipedal gait, dentition in the human direction, and the use of stone tools, and a relatively late development of the enlargement and elaboration of the brain.

In recent years a widely accepted view of the evolution of *Homo Sapiens* has been developed, although much remains to be ascertained. *Ramapithecus* is believed to have flourished from 14 to 10 million years ago (and perhaps later) in Asia and Africa; *Australopithecus* from 6 to 1 million years ago in Africa;* *Homo Erectus* from 1 million to 200,000 years ago, world-wide; and *Homo Sapiens* (including subspecies such as the Neanderthal) from 200,000 years ago to the present. The tendency has been for newer evidence to push back the time estimates for the emergence of the hominids. (For a sampling of work in physical anthropology, see John Buettner-Janush, *Origins of Man*, New York, Wiley, 1966; David Pilbeam, *The Ascent of Man*, New York, Macmillan, 1972; Stanley M. Garn, *Human Races*, 3rd ed., Springfield, Charles C Thomas, 1971; and Charles Hockett and Robert Ascher, "The Human Revolution," *Current Anthropology*, Vol. 5, 1964.)

Anthropological linguists have had considerable success in analyzing the structures of a large number of languages, which has been highly useful for fieldwork among the users of those languages. Much attention has been given to linguistic "salvage work"; the study of unrecorded or only partially recorded languages that are in danger of early extinction. Studies of the distribution of, and changes in, languages can be useful to nonlinguists; for example, archaeologists have used the distribution of Indian languages in South America as one indication of the movement of peoples. Although, as noted, much work in linguistics emphasized the structure of languages rather than what was communicated, recently many accounts emphasizing "semantic analysis" have been given. Much recent work has inquired into communicative behavior in the same general way that other forms of behavior are studied, and has attempted to place communication in its full ethnographic context. (For some representative work, see Dell Hymes, ed., *Language in Culture and Society*, New York, Harper & Row, 1964; Harry Hoijer, ed., *Language in Culture*, Chicago, University of Chicago Press, 1954; John J. Gumperz and Dell Hymes, eds., *Directions in Sociolinguistics*, New York, Holt, Rinehart, and Winston, 1972; and Benjamin N. Colby, "Ethnographic Semantics: A Preliminary Survey," *Current Anthropology*, Vol. 5, 1966.)

Useful discoveries have been made by archaeologists in practically every region of the world. In recent years much work has been done in areas somewhat neglected earlier, such as Africa, Southeast Asia, New Guinea, and the Pacific islands. Based on the remains, archaeologists have reconstructed the probable culture history of many groups. In addition to descriptions of the diet, subsistence, dwelling forms, settlement patterns, etc., of various groups, the "new archaeologists" have also attempted to describe general cultural processes in prehistoric populations, including social, political, and economic behavior. Hypothetical reconstructions based on the interconnections of cultural, ecological, and biological systems are emphasized by some archaeologists. (For some work, see Gordon R. Willey and Philip Phillips, *Method and Theory in American Archaeology*, Chicago, University of Chicago Press, 1958; Stuart Struever, ed., *Pre-Historic Agriculture*,

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*Recent work continues to modify earlier tentative conclusions. For example, as this Chapter was being written (Nov., 1972), Richard Leakey reported on a skull recently found in Kenya. His finding casts doubt on widely-held views of human evolution, and strongly suggests that *Australopithecus* should be excluded from the line of ancestry of *Homo Sapiens*.

Garden City, Natural History Press, 1971; and Don Brothwell and Eric Higgs, *Science in Archaeology*, rev. ed., New York, Praeger, 1970.)

Cultural anthropologists have studied the "ways of life" of many cultures and subcultures, with emphasis upon behavior in such areas as socioeconomic organization, family structures and relationships, personality structures, mythologies and systems of magic and religion, legal and other control systems, and art forms. The relation of particular cultural forms to the maintenance of the larger culture have been investigated. Cultural change has been intensively studied. In recent years, considerable attention has been given to *ethnicity*, including questions of the ways in which ethnic groups maintain a sense of identity and relate to other groups. Research on contemporary groups in cities (urban anthropology) has also become prominent; subcultures, such as a minority group or the poor, are studied in terms of their adaptive behavior to their marginal social position. (For examples, see Elliot Liebow, *Tally's Corner*, Boston, Little, Brown, 1967; Oscar Lewis, *The Children of Sánchez*, New York, Random House, 1961.)

A general theme in much of the work mentioned is that human patterns of adjustive behavior, to a much greater extent than those of other animals, are socially transmitted rather than biologically inherited, and may be considerably modified through the generations. (See Alan Lomax, with Norman Berkowitz, "The Evolutionary Taxonomy of Culture," *Science*, Vol. 177, July, 1972.) The anthologies edited by Yehudi A. Cohen *(Man in Adaptation*, Chicago, Aldine, 3 vols., 1968-1971) provide a convenient overview of much anthropological work.

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In addition to the four major subfields, other specialties are sometimes differentiated, such as medical anthropology, ethnomusicology, historical archaeology, anthropology and education, and ethnohistory. Such specialties often are organized in formal professional groups and publish their own journals. Applied anthropology frequently is considered as a fifth major subfield. (See discussion on applications in Smith and Fischer, *op. cit.*; and George M. Foster, *Applied Anthropology*, Boston, Little, Brown, 1969.)

5. CONTEMPORARY CONTROVERSY

Certain controversies concerning general procedures of inquiry were noted earlier. Some anthropologists argue for a plurality or diversity of methods, on the ground that anthropology may be enriched thereby and that a provocative set of competing "theories" may help in the accumulation of sufficient data to develop "compelling reconstructions." (Alan Howard, "Polynesian Social Stratification Revisted," *American Anthropologist*, Vol. 74, 1972, pp. 821-822.) In his *The Dialectic of Social Life* (New York, Basic Books, 1971), Robert F. Murphy has attempted to reconcile dialectically some of the conflicting points of view, by developing a new methodology. Others oppose the diversity of methods, and there is some tendency to discuss those issues in the context of philosophical points of view. Marvin Harris, for example, in his *The Rise of Anthropological Theory* (New York, Crowell, 1968) defends cultural materialism as opposed to idealistic interpretations of culture. According to Harris, the materialists emphasize evolution, the "materialistic reality" of culture, and logico-empirical methods, while the idealists (such as the ethnoscientists and the British-French tradition represented by Leach, Evans-Pritchard, and Lévi-Strauss) use nonscientific and subjectivist methods and regard culture as an abstraction.

For some time Leslie White's advocacy of cultural evolutionary theory was heavily criticized, and an almost standard point of view was that even the technologically simplest cultural groups of today could not be taken as representative of earlier evolutionary stages of development. More recently many anthropologists have adopted an evolutionary point of view, and often emphasize (as do the "cultural ecologists") the varying human adjustive responses to changing circumstances. (See Julian Steward, *Theory of Cultural Change: The Methodology of Multilinear Evolution*, Urbana, University of Illinois Press, 1955; Marshall D. Sahlins and Elman R. Service, eds., *Evolution and Culture*, Ann Arbor, University of Michigan, 1960.)

Some anthropologists argue that cultural phenomena are "autonomous" or "superorganic." Leslie White has probably been the strongest advocate of that point of view:

> "Customs and institutions—culture traits in general—constitute a distinct class of phenomena... culture *as culture* can be explained only in terms of culture. . . .In addition to the individual organic component in human behavior and over and above the social factor which comes from the interaction of individuals, there is the influence of the traditional customs, institutions, tools, philosophies, etc. These things, these culture traits, have an existence prior to the birth of any person living. They exist outside the human organism; they act upon him from the outside as meteorologic forces do." (White, *op. cit.*, pp. 78-79.)

Others regard that view of culture as hypostatization or even mysticism, and maintain that the cultural can be "reduced to" or "explained by" the social or the psychological. A.R. Radcliffe-Brown, for example, said:

> "You cannot have a science of culture. You can study culture only as a characteristic of a social system. Therefore, if you are going to have a science, it must be a science of social systems." (*A Natural Science of Society*, Glencoe, Free Press, 1957, p. 106.)

And Robert S. Lynd notes that "Culture does not enamel its fingernails, vote, or believe in capitalism, but people do." (*Knowledge for What?*, Princeton, Princeton University Press, 1939, p. 27.)

Recently there have been controversies about the comparative merits, the relations, etc., of "emic" and "etic" analyses. Both *etic* and *emic* are used in somewhat different ways, but the main core of the controversies seems similar to issues found in many of the behavioral areas of inquiry concerning contrasts between "inside" and "outside" techniques of observation. Emic analysis concerns the distinctions, categories, taxonomies, etc. (e.g., the way the color spectrum is divided) that are "subjectively meaningful" in a particular cultural grouping, and that may vary considerably from culture to culture. In etic analysis, the distinctions that are found to be scientifically useful are emphasized, even if they are not in accord with the subjective opinions of a cultural group. Emic inquirers are sometimes alleged to accept gullibly what they are told by informants, to mistake official ideologies for actual practices, or otherwise to confuse the objective and the subjective. On the other hand, defenders of emic inquiry argue that the native's beliefs, understandings, etc., are important, and need not be confused with anything else. (See Marvin Harris, *op. cit.*, pp. 568-604; Gerald D. Berreman, "Anemic and Emetic Analyses in Social Anthropology," *American Anthropologist*, Vol. 68, 1966; and Paul Kay, "Some Theoretical Implications of Ethnographic Semantics," in Ann Fischer, *op. cit.*)

The controversies just mentioned clearly involve divergent views about the objectives of inquiry in general, what "explanation" is, and other issues of the type discussed in Chapter 1.

Other long-standing controversies center on the degree of continuity between humans and other primates, with language being a central issue, inasmuch as humans everywhere, and almost uniquely, use language. Recent work on the communicative behavior of primates shows that an absolute difference between humans and non-humans is hard to maintain. The chimpanzee, Washoe, for example, was taught the sign language used by the deaf, and was able to combine those signs spontaneously and appropriately in the manner sometimes described as "true language." To illustrate, when she wanted the refrigerator opened, Washoe made the hand signs for *open-food-drink*, and when the alarm clock went off at meal time, she made the signs for *listen-eat*. (See R.A. and Beatrice T. Gardner, "Teaching Sign Language to a Chimpanzee," *Science*, Vol. 165, August, 1969.)

The controversies now tend to concern the differences in degree, rather than in kind, between humans and other primates, and how language developed. Some have argued that the development of language competence in humans was a gradual process extending over many millenia, while others have defended a single "great leap" view. (See Charles F. Hockett and Robert Ascher, "The Human Revolution," *Current Anthropology*, Vol. 5, 1964; and Eric H. Lenneberg, "Language, Evolution, and Purposive Behavior," in Stanley Diamond, ed., *Culture in History*, New York, Columbia University Press, 1960.)

Some inquirers believe they have found evidence showing that the members of relatively simple social groups tend to think less abstractly than individuals in more complex groups, or simply lack the capacity for abstract thought. (See Brent Berlin, "A Universalist-Evolutionary Approach in Ethnographic Semantics," in Ann Fischer, *op. cit.*, pp. 14-15; and John Gay and Michael Cole, *The New Mathematics and an Old Culture*, New York, Holt, Rinehart and Winston, 1967.) On the other hand, Gary J. Witherspoon believes that the Navajo have a highly developed capacity for abstraction, and he suggests that, rather than the thinking of people in simple societies being primitive, it is our understanding of them that is primitive. ("Navajo Categories of Objects at Rest," *American Anthropologist*, Vol. 73, 1971, p. 121.)

In addition to the type of controversy just considered, anthropologists recently have been involved in the type of heated debate found in many other fields concerning the possible political uses and relevance of their work, the ethical responsibilities of anthropologists in relation to the people they are studying, the question of whether "objective" research is possible in principle, etc. The *Newsletter* of the American Anthropological Association, for example, contained many strongly-worded discussions of such matters in 1971 and 1972, and at the recent annual meetings of the Association much time has been spent in discussion and voting on political matters. (For the activist point of view, see the papers by Gerald D. Berreman, Kathleen Gough, and Gutorm Gjessing in *Current Anthropology*, Vol. 9, 1968.) In addition, members of some of the cultures much studied by anthropologists, such as the American Indians, recently have protested the "invasion" of anthropologists and have maintained that the findings of anthropologists have been distorted, demeaning, patronizing, and chauvinistic.

6. TERMINOLOGICAL PROBLEMS

As in many other areas, there is confusion and disagreement concerning some of the words used frequently in general discussions of the procedures of inquiry. "Scientific," for example, is used to designate quite different types of procedures. The British social anthropologists are sometimes taken as representative of highly developed scientific procedures, and yet have also been severely criticized as nonscientific:

> "The misunderstanding of scientific method is perhaps most extreme in that group of anthropologists which makes the most vociferous pretensions to being scientific and comparative—the British structuralists headed by Radcliffe-Brown. The alleged 'laws' of this school turn out, upon examination, to be verbal statements like 'the equivalence of brothers' or 'the necessity for social integration' which fail completely to specify the concomitant behavior of variables. . . ." (Murdock, *op. cit.*, p. 27.)

"Explanation" has also proved troublesome, especially when an explanation telling *why* something happened is opposed to a description telling *what* happened. Some "explanations" are tautological, as when Ralph Linton asserted that the Plains Indians fought many wars, not to obtain hunting grounds or the like, but because they were "warlike." (*The Study of Man*, New York, Appleton-Century, 1936, p. 461.) Other "explanations," intended to be scientific and useful, are descriptive in the sense in which we use that word, for they describe what happens under specified circumstances.

To illustrate, Leslie White, after criticizing attempts to "reduce" the cultural to the psychological, says:

> "To the culturologist the reasoning that says that one people drinks milk because 'they like it,' another does not because 'they loathe it,' is senseless; it explains nothing at all. Why does one people like, another loathe, milk? *This* is what we want to know. And the psychologist cannot give us the answer. Nor can he tell us why a people does or does not avoid mothers-in-law; practice monogamy, inhumation, the couvade, or circumcision; use chopsticks, forks, the pentatonic scale, hats or microscopes; form plurals by affixation—or any of the other thousands of customs. . . .The culturologist explains the behavior of a people by pointing out that it is merely *the response of a particular type of primate organism to a particular set of stimuli.* And he explains culture along the lines indicated earlier [human behavior has two separate and independent sources—the biological and the socially transmitted supra-biological, or cultural]. Thus, while the culturologist is quite willing to admit that it is people who 'enamel their fingernails' or drink milk, he desires to point out that whether they do or not is determined not by themselves but

by their culture." (White, *op. cit.*, pp. 143-144; italics added to the long phrase.)

The "why" answer provided by White is no more than a description of what happens under specified circumstances. His argument, if correct, shows that much human behavior varies according to the cultural field, but it does not provide an explanation that goes "beyond" description.

The quotation from White also relates to other terminological problems concerning "levels of behavior." Some inquirers conclude or assume that there are four separate but interacting levels: the biological, the psychological, the social, and the cultural. Others deny the existence of the cultural as a separate level, but retain the other three; while yet others make some other differentiation. Such procedures almost inevitably involve the old difficulties of how to provide coherently both for "separation" and for "interaction," and tend to encourage futile debate similar to the ancient discussions concerning hierarchically arranged kinds of being.

Many of the key names used within the field of anthropology are applied in diverse ways; there is considerable vagueness and ambiguity in the uses of "culture," "lineage," "descent," "structure," and "function." Kroeber and Kluckhohn, for example, found 164 "definitions" of "culture." ("Culture: A Critical Review of Concepts and Definitions," Cambridge, *Papers of the Peabody Museum*, Vol. XLVII, No. 1a, 1952.) And William E. Mitchell has argued that the different and conflicting ways of using "kindred" raise serious difficulties, particularly in the cross-cultural description of the structural aspects of kindreds. ("Theoretical Problems in the Concept of Kindred," *American Anthropologist*, Vol. 65, 1963.) Such disagreements about naming are enmeshed in the controversies about procedures of inquiry and the objectives of anthropological research.

The situation is exacerbated by the unusually broad array of methods used by anthropologists and the borrowing of names from everyday speech ("family," "marriage," "function," "pattern," etc.), often with relatively little or no progress toward useful scientific naming or specifying. Moreover, the language system native to the anthropologist may impose a kind of "strait-jacket" in the study of cultures very unlike the anthropologist's own, as is suggested by the Sapir-Whorf hypothesis. (See Benjamin L. Whorf, *Language, Thought and Reality*, edited by J.B. Carroll, New York, Wiley, 1956.)

Comparing highly diverse cultures requires technical naming and ways of categorizing that are appropriate for cross-cultural observations. A beginning was made by Murdock and others in the Human Relations Area Files, in which data from a sizeable number of cultures is classified. However, the categories used reflect the earlier interests of comparativists, and the Files do not include much recent data. (See George P. Murdock *et. al.*, *Outline of Cultural Materials*, 4th rev. ed., New Haven, Human Relations Area Files, 1961.)

7. COMMENT AND EVALUATION

More often than do inquirers in other fields, anthropologists view human adjustive behavior as *bio-social*, thus avoiding considerable obscurantism and many needless puzzles that follow from dualistic views of human nature. As was noted earlier, the "holistic" procedures characteristic of much anthropological inquiry are partially transactional. In our opinion, however, the full benefits of such procedures will not be obtained as long as anthropologists emphasize "hypothetical reconstructions" based on insufficient data and on the self-actional and interactional procedures criticized in Chapter 1.

Another way of making this point is to note the institutionalized tolerance in the field of anthropology for subjective and speculative methods. According to Pelto:

> ". . .I have been suggesting. . .that the apparent weaknesses of anthropological work derive much less from the inherent difficulties of our subject matter, and much more from persistently nonproductive features of our anthropological subculture. These nonproductive features are perpetuated through direct transmission within our programs of graduate instruction. They are also perpetuated by some very general tendencies in the institutional make-up of the social sciences. I have written this book with the hope of furthering a pattern of culture change which already seems to be gathering momentum. Possibly new methodological developments will arise that can bring about a real 'revitalization movement.' " (Pelto, *op. cit.*, p. 316.)

Although many anthropologists have urged the use of scientific procedures of inquiry, quite often they include as scientific elaborate "theorizing" far in advance of observation. To illustrate, in his final chapter Pelto advocates the use of multiple hypotheses that are derived from a theoretical point of view. Those alternative hypotheses are then tested against the data, and the process is continued until only one hypothesis remains. Pelto goes on to complain that so far only the most bizarre hypotheses have been eliminated, which is about what one would expect. After all, human ingenuity has a proven record for concocting plausible conjectures, and it seems probable that such conjectures can be imagined faster than they can be tested.

We suggest that the procedures described in Chapter 1 are more useful and would tend to free anthropological inquiry from many of the controversies and blind alleys generated by the self-actional and interactional procedures now frequently used.

One need not postulate a separate level (logical, ontological, or other) for culture in order to emphasize the importance in human behavior of socially transmitted culture traits and patterns, thereby avoiding a mare's nest of issues about the possible connections of various levels. Nor does one have to opt for an "inner," an "outer," or a combination "inner-outer" procedure for studying behavior; in knowing-naming transactions the inquirer is always connected in many ways with aspects and phases of the field into which he is inquiring. Nor, finally, does inquiry into sign-behavior (so important in the transmission, acquiring, and modification of culture traits) require procedures relying on subjective "meanings," motives, intentions, etc.

To illustrate in one specific instance, some of the controversies concerning the emic-etic distinction appear to have little point. Rather than two basically opposed methods, both may be appropriate and useful in different phases of inquiry. "Emic analyses" may be particularly useful in "cracking the code" used within a culture, but because a particular number of gods is differentiated within a given culture does not demonstrate that those gods exist. More generally, the ways of differentiating and classifying characteristic of any human group may not be as useful as the ways developed through scientific inquiry. An "emic analysis" would presumably show that whales are classified as fish by many people, but an "etic analysis" classifying whales as mammals is not thereby refuted. In short, inquiry into the beliefs about the cosmos shared by a group may be highly useful in describing that culture, but obviously those beliefs need not be warranted.

There are some indications that anthropologists are giving increasing attention to the requirements of scientific inquiry, as noted throughout this Chapter. Possibly the "revitalization movement" mentioned by Pelto will occur. On the other hand, in many behavioral fields in recent years a retrogression has occurred in which many inquirers have turned away from scientific procedures, and perhaps the most characteristic procedures used in the behavioral fields during the next decade will be nonscientific ones.

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ANTHROPOLOGICAL LINGUISTICS
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ARCHAEOLOGY
ASIAN PERSPECTIVES
COMPARATIVE STUDIES IN SOCIETY AND HISTORY
CURRENT ANTHROPOLOGY
ETHNOHISTORY
ETHNOLOGY
FOLKLORE
HUMAN BIOLOGY
HUMAN ECOLOGY
HUMAN ORGANIZATION
HUMAN RELATIONS
INTERNATIONAL BIBLIOGRAPHY OF SOCIAL AND CULTURAL ANTHROPOLOGY
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MAN
OCEANIA
SOUTHWESTERN JOURNAL OF ANTHROPOLOGY
URBAN ANTHROPOLOGY

IV.

SOCIOLOGY*

1. WORKING DESCRIPTION OF THE FIELD

SOCIOLOGISTS inquire into the behavior of people in groups and organizations, including the patterns, regularities, variations, and developmental changes in human relations, customs, and institutions. Sociological inquiry overlaps considerably with inquiries made by cultural anthropologists and psychologists, and shares partially the subject matter of many other behavioral fields.

2. OTHER DESCRIPTIONS OF THE FIELD

Sociology as a distinct field gradually emerged during the nineteenth century. Auguste Comte was the first to use the label "sociology" and advocated a scientific ("positive"), rather than a theological or metaphysical, study of society. Herbert Spencer applied the theory of biological evolution to social processes. Emile Durkheim attempted to deal with "social facts" as an emergent level of phenomena not reducible to the biological or psychological levels. Max Weber and Georg Simmel held that sociology was a "generalizing science" concerned with "common value orientations" and "ideal types."

Among the early influential sociologists in America were Lester F. Ward, William Graham Sumner, Albion Small, Franklin H. Giddings, and Charles H. Cooley. Sociology was often regarded as the sole social science, as a synthesis of the separate social sciences, or as the basic social science.

Some typical recent efforts to describe the field of sociology follow.

Sociology is "the discipline that describes the phenomena that are created by the social interaction of human beings and the manner in which these phenomena affect the behavior of individuals." (Don Martindale and E.D. Monchesi, *Elements of Sociology*, New York, Harper, 1951, p. 39.)

Sociology is "a body of related generalizations about human social behavior arrived at by scientific methods." (George A. Lundberg, C.C. Schrag, and O.N. Larsen, *Sociology*, rev. ed., New York, Harper, 1958, pp. 6-7.)

Sociology is "the science which studies the structure and function of social relations, customs, and institutions in different groups, and the processes by which they change." (Eva J. Ross, *Basic Sociology*, rev. ed., Milwaukee, Bruce, 1958, p. 4.)

Arnold Rose sees sociology as "the science of human relations" (*Sociology: The Study of Human Relations*, New York, Knopf, 1956), and James A. Quinn as "the general science of human social groups" (*Sociology: A Systematic Analysis*, New York, Lippincott, 1963).

A report jointly sponsored by the National Academy of Sciences and the Social Science Research Council mentions five aspects of social life as constituting "the major perspectives of sociology": 1) demographic and ecological patterns of human populations, such as birth, death, migration, spatial arrangement, etc.; 2) social-psychological phenomena, such as small group interaction patterns, interaction of personality and society in socialization, etc.; 3) collective behavior in groups and organizations; 4) structural relations arising in social interaction, as in the patterns of roles in a political structure; and 5) cultural phenomena such as norms, values, and ideologies that tend to regulate or legitimize social behavior. They go on to say:

> "The subject matter of sociology, then, is found in the demographic-ecological, social-psychological, collective, structural, and cultural aspects of social life. The sociological enterprise is to explain regularities, variations, and interdependencies among those aspects. This enterprise has both a static and a dynamic aspect. Sometimes sociologists ask why patterns of organized social life persist, but equally often they are concerned with processes of social change, which destroy old social forms and create new ones." (Neil J. Smelser and James A. Davis, *Sociology*, Englewood Cliffs, Prentice-Hall, 1969, pp. 31-32.)

3. METHODS AND TYPES OF INQUIRY

When the first edition of this volume was written, the procedures of inquiry called the "natural science approach" tended to dominate American sociological inquiry. In the intervening years, sociologists critical of scientific procedures of inquiry have become much more numerous, and many controversies concerning scientific method and its applicability to sociological problems have again become prominent. Kurt W. Back describes the current scene as follows:

> "From the number of remedies recommended for the current situation of sociology, one must conclude that this field is very sick. Advice on diagnosis and therapy comes from all sides. Some claim that sociology is not a strict enough science in following empirical logical procedures, and see the remedy as improvement of mathematical and logical procedures. Others accuse sociology and especially sociologists of not being sensitive enough to the crucial problems of the present time, and of pursuing the chimera of value-free science, suggesting instead a methodology of engagement such as Neo-Marxism or Gouldner's reflexive sociology. Still others look to participant observation and to extracting the symbolic meaning from the behavior and the language of the actors themselves. . . . Of the critics, one side wants to adopt the method and procedure of the hard sciences and use the language of mathematics; another wants to adopt the method and procedure of social action and use the language of the activists and propagandists; and the third wants to adopt the philosophical approach of the existentialists and phenomenologists and use the language of the novelists." (Review of Robert L. Burgess and Don Bushnell, Jr., *Behavioral Sociology*, New York, Columbia University Press, 1969, in *American Sociological Review*, Vol. 35, 1970, p. 1098.)

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* We are grateful to George A. Theodorson and Martin H. Neumeyer for their comments concerning this chapter. No part of the chapter, as revised, necessarily represents either of their points of view.

The major controversies will be discussed in more detail in Section 5 of this Chapter. In the present section we will consider some of the major points of view about the field and the most useful procedures for inquiry into its subject matter.

Natural Science Procedures

Defenders of this type of inquiry argue that sociologists should apply the general methods (but not necessarily the specific techniques and instruments) of the natural sciences to human behavior. Among the leading exponents were George A. Lundberg, Read Bain, Stuart C. Dodd, Samuel Stouffer, and Paul F. Lazarsfeld. "Science" and related names are often used in different ways, and differing emphases are found within this general point of view. Some critics regarded the natural science sociologists as "neopositivists," because of certain similarities to Comte's views and to those of the later logical positivists.

In the 1950's the natural science viewpoint was often described by both its defenders and critics as the dominant trend in American sociology. (George Lundberg, "The Natural Science Trend in Sociology," *American Journal of Sociology*, Vol. LXI, 1955; N.F. Timasheff, *Sociological Theory*, Garden City, Doubleday, 1955, Ch. 15.) Even then there were many critics; the earlier critics often viewed behaviorism, pragmatism, and quantitativism as the three-fold basis of the natural science trend. (See Timasheff, *op. cit.*, p. 137, and Preston Valien and Bonita Valien, "General Sociological Theories of Current Reference," in Howard Becker and Alvin Boskoff, eds., *Modern Sociological Theory*, New York, Dryden, 1957, p. 86.)

Lundberg pointed out that behaviorism does not exclude inquiry into so-called "mental" processes, which he regarded as symbolic or verbal forms of behavior. Rather than ignore or declare "unreal" such behavior, the natural science sociologists tried to describe it scientifically. What the critics referred to as "pragmatism" is not always clear. Some may have been objecting to Dewey's insistence that the "mental" can be inquired into using scientific procedures. Or they may have opposed the Peirce-Dewey view that all scientific statements are subject to possible future modification, correction, or rejection, and that there are no known absolutes in science. Broadly speaking, the "pragmatists" insist that all conjectures or hypotheses are to be tested by reference to their consequences and that methodological rules and procedures are to be evaluated in terms of their usefulness in inquiry.

Quantification is emphasized by the natural science sociologists in the development of scales and other measuring devices; the use of statistical analyses, surveys involving sampling, interviewing, questionnaire construction, and complex tabulation; and in systematizing methods of quantitative observation. (See Paul F. Lazarsfeld and Morris Rosenberg, eds., *The Language of Social Research*, Glencoe, Free Press, 1955.)

As an example of one elaborate attempt to quantify, Stuart C. Dodd constructed "a quantitative systematics for the social sciences" in his *Dimensions of Society* (New York, Macmillan, 1942). He introduced "the S-theory," later renamed the "S-system," for standardized description in algebraic formulae:

> "Any quantitatively recorded societal situation can be expressed as a combination of indices of *time*, of *characteristics* of people or of their environments, of *space*, and of *population*, modified by *exponents*, and by three other scripts specifying the kind and number of *classes*, *class-intervals*, and *cases* of what the index denotes, and combined by the signs for *adding*, *subtracting*, *multiplying*, *dividing*, *aggregating*, *cross-classifying*, *correlating*, and *identifying*." (*Ibid.*, p. 26.)

Dodd later developed his system further; see, for example, "The Transact Model" (*Sociometry*, Vol. LXVIII, 1955).

Despite the strong emphasis on quantification, Lundberg cautioned against exaggerating its importance. He said:

> "...we hold that 'quantitativism' is merely a particular way of observing, recording, and manipulating data.... Quantification is only a way of expressing degrees of qualities and relationships.... We have never thought of either quantification or operationism as entirely supplanting or preventing other forms of thinking...." (Lundberg, 1955, *op. cit.*, pp. 192-193.)

More recent critics, although making some of the same objections as the earlier critics, emphasize charges such as that the natural science trend is trivial, artificial, and distorts, or is a flight from, reality. Some illustrations of such criticisms follow.

> "The import of this natural science approach to the subject matter of sociology is that sociologists have tended to bend, re-shape, and distort the empirical social world to fit the model they use to investigate it. Wherever possible, social reality is ignored. Most sociologists seem to have forgotten that reality exists only in the empirical world and not in the methods sociologists use to measure it. I can find no methodological or epistemological justification that would support the natural science model as being the best model for presentation of the empirical social world." (William J. Filstead, ed., *Qualitative Methodology*, Chicago, Markham, 1970, p. 3.)

Irving L. Horowitz criticizes the "transformation of sociology into scientism," maintains that "methodology became a substitute for social problems" and empiricism a way of avoiding moral issues, and argues that the specialized techniques used often become the end of research rather than instruments of research. (*The New Sociology*, New York, Oxford University Press, 1964, p. 6, p. 9.)

Irwin Deutscher says:

> "In attempting to assume the stance of physical science, we have necessarily assumed its epistemology, its assumptions about the nature of knowledge and the appropriate means of knowing, including the rules of scientific evidence.... One of the consequences of using the natural science model was to break down human behavior in a way that was not only artificial but which did not jibe with the manner in which behavior was observed." ("Words and Deeds: Social Science and Social Policy," *Social Problems*, Vol. 13, 1966, p. 241.)

Pitirim A. Sorokin, whose views earlier were often ignored but who apparently is now regaining influence, notes that in the late nineteenth and early twentieth century sociologists were synthesizers and generalizers, but for forty or fifty years thereafter sociologists emphasized fact-finding. He then says:

> "Preoccupied mainly with techniques, narrow concrete problems and analytical theorizing, detached from empirical realities, recent sociology has neither produced a great synthesis nor discovered a great, empirical uniformity. Its theories and research represent mainly reiteration, variation, refinement, and verification of methods and theories developed by sociologists of the preceding period. Through empirical research, recent sociology has given us a fuller knowledge of a few 'specks' and dimensions of the total, immense, multidimensional sociocultural reality but it has not substantially increased our understanding of the total 'superorganic' reality. If sociology is going to grow as a basic science of sociocultural phenomena, it is bound to pass into a new synthesizing-generalizing phase." ("Sociology of Yesterday, Today and Tomorrow," *American Sociological Review*, Vol. 30, 1965, p. 833.)

In many of the "new" sociologists there is a kind of romantic voluntarism. Alvin Gouldner, for example, emphasizes self-awareness, accepting the validity of one's sentiments, and trusting one's authentic impulses. The "reflexive sociology" he advocates has the following objective:

> "...the historical mission of a Reflexive Sociology as I conceive it...would be to *transform* the sociologist, to penetrate deeply into his daily life and work, enriching them with new sensitivities, and to raise the sociologist's self-awareness to a new historical level." (*The Coming Crisis of Western Sociology*, New York, Basic Books, 1970, p. 489.)

Social-Action Theory

Social-action theory is sometimes labeled "analytical sociology," and focuses on the expenditure of effort by a group; i.e., on collective endeavor, social interaction, and social systems. Florian Znaniecki, Robert M. MacIver, Howard S. Becker, and Talcott Parsons were some of the leading social-actionists. (See R.C. Hinkle, Jr., and Gisela J. Hinkle, *The Development of Modern Sociology*, Garden City, Doubleday, 1954.) Ends and norms, as well as means and conditions, were emphasized as basic constituents of social-action systems.

The supporters of social-action theory often were influenced by Max Weber's *verstehende* sociology. Such writers emphasize the *meaning* of social behavior through an *understanding* of the subjective perspective of the behaver. According to John C. McKinney: "The emphasis was on *understanding* social behavior, which meant that mere statistical regularities had to be supplemented by knowledge of the subjective motivation before causality could be imputed *on the level of meaning.*" ("Methodology, Procedures, and Techniques in Sociology," in Becker and Boskoff, *op. cit.*, p. 196.) A decade or two ago sociologists seemed to be abandoning *verstehen* procedures, but recently there has been a marked revival. (See Leonard S. Krimerman, ed., *The Nature and Scope of Social Science*, New York, Appleton-Century-Crofts, 1969.)

Talcott Parsons and his followers also advocated systematic "theory construction." According to Parsons:

> "Theory...in the scientific sense, consists in a logically related integrated set of propositions about the relations of variables, that is, abstract conceptual entities, in terms of which many statements of fact can be systematically related to each other, and their meanings for the solution of empirical problems interpreted. Besides the all-important empirical relevance, the principle criteria of good theory are conceptual clarity and precision and logical integration in the sense not only of the logical compatibility of the various propositions included in a theoretical scheme, but of their mutual support, so that inference from one part of the scheme to other parts becomes possible." ("Comment" on "Preface to a Metatheoretical Framework for Sociology," *American Journal of Sociology*, Vol. LXVII, 1961, p. 137.)

Functionalism

Many consider functionalism to be a major trend among recent American sociologists, but functionalism is not always easily distinguishable from social-action and other types of inquiry. Several important sociologists are closely identified with functional analysis, including Robert K. Merton (*Social Theory and Social Structure*, rev. ed., Glencoe, Free Press, 1957) and Talcott Parsons, as well as others who emphasize social-action. (See also Marion J. Levy, Jr., *The Structure of Society*, Princeton, Princeton University Press, 1952, and F. Stuart Chapin, *Contemporary American Institutions*, New York, Harper, 1935.)

A functional analysis is said to be a study of social phenomena as operations within, or effects of, specified social structures (e.g., a class system, a kinship group). Merton says "functions" are "those observed consequences which make for the adaptation or adjustment of a given system." (Merton, *op. cit.*, p. 50.) The parts of a social system are viewed as interdependent and as contributing, on balance, to the maintenance and integration of the whole system. Merton allows also for "dysfunctions" in regard to the total system and its subsystems or parts. The "equilibrium" of a system is emphasized by functionalists. Some interpret an equilibrium in static terms, others in the form of a dynamic unity. Merton distinguishes between "manifest functions" (the objective consequences of a social or cultural unit as recognized by the participants) and "latent functions" (the unintended and unrecognized consequences). The functionalists claim to find important functional analyses in anthropology, psychology, and other fields.

Sometimes functionalism is said to be a special method of inquiry, the use of which differentiates the social from the physical sciences. Ernest Nagel rejects such a view, pointing out that the type of process the functionalists emphasize is found also in the physical sciences; e.g., a thermostat's operation is "functional." (*The Structure of Science*, New York, Harcourt, Brace, 1961, Chs. 12, 14.) Kingsley Davis has argued that all sociologists inquire into both *structures*, as found at any given time, and *functions*, construed as time series and consequences of structures. ("The Myth of Functional Analysis as a Special Method in Sociology and Anthropology," *American Sociological Review*, Vol. 24, 1959.)

Recently some critics have argued that the functionalists, with their emphasis on equilibria, give political and ideological support to the existing socioeconomic arrangements. Alvin Gouldner, for example, sees Parsons as exemplifying establishment sociology and says that the functionalists "are conscientious 'guardians' devoted to the maintenance of the social machinery of whatever industrial society they are called upon to service." (Gouldner, *op. cit.*, p. 332.)

Ideal Type Theory

Harry E. Moore says an "ideal type" is:

> "A configuration or gestalt of characteristics constructed by bringing together those most often observed in specimens of the category under consideration. . . . It must be observed that 'ideal' as here used carries no connotation of 'better' or 'poorer,' i.e., is entirely non-normative." (In Henry Pratt Fairchild, ed., *Dictionary of Sociology*, New York, Philosophical Library, 1944.)

The development of ideal types was emphasized by Max Weber in order "to analyze historically unique configurations or their individual components by means of genetic concepts." He used the ideal types of *church* and *sect* to inquire into the Christianity of the Middle Ages and to relate Protestantism to the rise of capitalism. (*The Methodology of the Social Sciences*, translated and edited by Edward A. Shils and Henry A. Finch, Glencoe, Free Press, 1949, p. 93.)

Howard S. Becker prefers the name "constructed type" to "ideal type." He regards the types as end products of research that may have predictive power and that help to clarify our accounts of the phenomena involved. (*Through Values to Social Interpretation*, Durham, Duke University Press, 1950.)

Ideal type theory has recently been viewed by some as "model building" that specifies in precise detail what the type is and how it can be applied. Don Martindale points out that much ambiguity prevails whether the types are taken as "theories" or "models." ("Sociological Theory and the Ideal Type," in Llewellyn Z. Gross, ed., *Symposium on Sociological Theory*, Evanston, Row, Peterson, 1959.) For a discussion of some of the uses and abuses of models, see May Brodbeck, "Models, Meaning, and Theories" (in Gross, *op. cit.*)

Joseph Lopreato and Letitia Alston find attempts to use ideal types are often "self-defeating" and produce "indescribable confusion." They recommend that "ideal type" be dropped from the vocabulary of sociologists and be replaced by "idealization," "research model," or "guiding scheme." ("Ideal Types and the Idealization Strategy," *American Sociological Review*, Vol. 35, 1970.)

Qualitative Methods

Much of the opposition to the procedures of inquiry suggested in Chapter I is illustrated in the book *Qualitative Methodology*, edited by Filstead, who says:

> "We are gaining technical specialities with little thought as to their usefulness in terms of assaying the reality of the empirical social world. This increasing trend toward quantification has led to a lessened understanding of the empirical social world. The artificial conception of reality that is fostered in present-day theoretical, methodological, and conceptual schemes results in the paucity of explanatory schemes of human behavior. In order to increase their understanding of human behavior, sociologists must become, not more detached from, but more involved with the phenomena of the empirical social world." (Filstead, *op. cit.*, p. 2.)

Filstead mentions Herbert Blumer, Irwin Deutscher, Marshall B. Clinard, Severyn Bruyn, Howard S. Becker, Alvin Gouldner, and Irving Horowitz as important critics of positivistic sociology, and Harold Garfinkel, David Sudnow, Donald W. Ball, Howard S. Becker, Sherri Cavan, Ned Polsky, Barney Glaser, Anselm Strauss, Fred Davis, and Julius Roth as qualitative methodologists. Such writers are interested in *verstehen*, "sympathetic introspection," and the use of participant observer and phenomenological methods.

Often a sharp separation is made between the physical and the behavioral sciences. Krimerman (*op. cit.*) believes that the explanation of human action is different from physical science explanations, emphasizes the voluntary nature of human action, regards *verstehen* as an indispensable social science method, sees objectivity as impossible in the social sciences, etc.

Severyn Bruyn believes that physical science inquirers use an outer, behavioristic, and physicalistic type of procedure, while humanists study human meanings in an inner, personal-social, experiential way. Sociological inquiry, he says, can perform a synthesis; a participant observer (e.g., a person who studies a street gang while he is a member of it) can emphasize the "inner" as he studies "lived experience" rather than "scientific abstractions and reductionisms." (*The Human Perspective in Sociology*, Englewood Cliffs, Prentice-Hall, 1966, and "The New Empiricists: The Participant Observer and Phenomenologist," *Sociology and Social Research*, Vol. 51, 1967.)

As in psychology, phenomenological and existentialist procedures of inquiry are also advocated. Edward A. Tiryakian sees existential phenomenology as related to the sociological tradition of subjective realism or *verstehen*. ("Existential Phenomenology and the Sociological Tradition," *American Sociological Review*, Vol. 30, 1965. See also John C. McKinney and Edward A. Tiryakian, eds., *Theoretical Sociology*, New York, Appleton-Century-Crofts, 1970.) Bruyn notes similarities between the phenomenological and participant observer methods and says:

> "The phenomenologist and the participant observer. . .are both taking man as he is given in his lived experience. They are placing the mechanical, organic, and functional images of man in their proper perspective–not negating their value to the formulation of theory but denying their supremacy in the explanation of society. They are giving supremacy to an inner perspective of man in society which ultimately could lead toward a more comprehensive sociological perspective." (Bruyn, 1967, *op. cit.*, p. 322.)

Such methods are similar in some respects to those used by the *symbolic interactionists* (whose work is discussed in Section 4) and the *ethnomethodologists*. The ethnomethodologists, influenced by the phenomenological philosophy of Alfred Schutz, inquire into the "rational properties" of the taken-for-granted expectations and understandings found in the everyday activities of the members of a particular social group. (See Harold

Garfinkel, *Studies in Ethnomethodology*, Englewood Cliffs, Prentice-Hall, 1967; and the Review Symposium in *American Sociological Review*, Vol. 33, 1968, especially the scathing criticisms by James S. Coleman.)

Until recently, extreme antiscientific views maintaining that there could be no successful scientific inquiry into much human behavior because humans are radically unlike the proper subject matter of scientific inquiry, that free will makes prediction impossible, etc., were found primarily in writers outside of sociology (for example, Peter Winch, *The Idea of a Social Science and its Relation to Philosophy*, London, Routledge & Kegan Paul, 1958), but now such views are re-emerging in sociology. (For a general critique of such views, see Felix Kaufmann, *Methodology of the Social Sciences*, New York, Oxford University Press, 1944, pp. 141-147, and Rollo Handy, *Methodology of the Behavioral Sciences*, Springfield, Charles C Thomas, 1964.)

4. RESULTS ACHIEVED

When the first edition of this book was written, there seemed to be a marked convergence at least in research methods among sociologists holding diverse opinions on other matters. In 1955, for example, George Lundberg was impressed by the convergences in the work of sociologists such as Parsons, Merton, and Bales and the work of Stouffer, Lundberg, Dodd, and Lazarsfeld. (Lundberg, 1955, *op. cit.*) As we have noted, however, more recently many sociologists have begun to advocate and use "inner" procedures of inquiry; possibly the immediate future will show an even greater divergence in the main procedures of inquiry used by sociologists.

A detailed account of all current research in sociology is impracticable here. Some of the major lines of work are indicated below.

Demography and Ecology

Demography is the study of the sizes, distributions, and compositions of populations. Demographers use precise quantitative methods and have made predictive extrapolations of population trends based on birth rates, death rates, and migration patterns. Demographic studies are related to economics, biology, and medicine; they are also related to studies of psychological and social processes associated with population characteristics. The development of computers has been highly useful for many demographic inquiries. (For work in demography, see Richard N. Farmer, John D. Long, and George J. Stolnitz, eds., *World Population–The View Ahead*, Bloomington, Indiana Bureau of Business Research, 1968; Joseph Spengler and Otis D. Duncan, eds., *Population Theory and Policy*, Glencoe, Free Press, 1956.)

Studies in the area of human ecology inquire into the relation between important aspects of individual behavior and spatial areas that are homogeneous in important ways. For a recent example, see Gerald T. Slatin, "Ecological Analyses of Delinquency: Aggregation Effects" (*American Sociological Review*, Vol. 34, 1969).

Much work in urban sociology (concerning the ways metropolitan areas change and develop) and in rural sociology (concerning the diffusion of farm practices, migration, labor force, etc.) is closely related to demographic and ecological investigations.

Social Psychology

Typically work in this area uses an interactionist framework and considers the interactions of personality characteristics and social and cultural variables, including studies of attitudes, values, and beliefs. (See, for example, Arnold Rose, ed., *Human Behavior and Social Processes: An Interactionist Approach*, Boston, Houghton Mifflin, 1962.) Sometimes "interaction" is applied as we use the name in Chapter I, and sometimes as closer to what we call "transaction." Considerable attention is given to how children and adults are socialized, i.e., how they learn to adjust to the social order through both conformist and nonconformist behavior patterns. (See John A. Clausen, ed., *Socialization and Society*, Boston, Little, Brown, 1968, for a report on much recent work.) A general overview of work in social psychology can be found in Leonard Berkowitz, ed., *Advances in Experimental Social Psychology* (New York, Academic Press, 1967) and in Henry C. Lindgren, ed., *Contemporary Research in Social Psychology* (New York, Wiley, 1969).

Much interest has been shown in "symbolic interactionism." The symbolic interactionists are interested in how individuals interpret situations and communicate with each other. They attempt to relate overt behavioral interactions to covert symbolic behavior, with an emphasis on the *meanings* objects acquire in those interactions. Frequently the *self* and the subjective are emphasized, and natural science procedures of inquiry into human behavior are rejected. There are also links to social-action theory. (For a sampling of work in this area, see Jerome G. Manis and Bernard N. Meltzer, eds., *Symbolic Interaction: A Reader in Social Psychology*, Boston, Allyn and Bacon, 1967; Herbert Blumer, *Symbolic Interactionism*, Englewood Cliffs, Prentice-Hall, 1969; Charles K. Warriner, *The Emergence of Society*, Homewood, Dorsey, 1970; Hugh Dalziel Duncan, *Symbols in Society*, New York, Oxford University Press, 1968; and Manford H. Kuhn, "Major Trends in Symbolic Interaction Theory in the Past Twenty-Five Years," *Sociological Quarterly*, Vol. 5, 1964.)

Groups, Organizations, Institutions, and Stratification

A central concern of sociologists is the investigation of human behavior in organized groups (primary groups, voluntary associations, formal organizations, and whole societies), including the roles individuals play, the structure of the groups, stratification and differentiation within groups, and the influence of social institutions on behavior.

A great deal of effort has gone into the study of small groups such as the family. An influential early study was Jacob Moreno's *Who Shall Survive?* (Washington, D.C., Nervous and Mental Disease Monographs, 58, 1934). A more recent representative collection of work done is found in *Small Groups*, edited by A. Paul Hare, Edgar F. Borgatta, and Robert F. Bales (New York, Knopf, 1955). Theodore Mills' *The Sociology of Small Groups* (Englewood Cliffs, Prentice-Hall, 1967) contains a history of small group research and compares the results of laboratory studies unfavorably to what can be learned from the experience of leaders who are part of "self-analytic" training or therapy groups.

Studies of formal organizations and bureaucracies have also received much attention. One procedure involves case studies and focuses upon internal comparisons among the segments of an organization. (See Seymour M. Lipset, M.A. Trow, and J.S. Coleman, *Union Democracy: The Internal Politics of the International Typographical Union*, Glencoe, Free Press, 1956.) Another procedure is to emphasize a systematic comparison of organizations. Some have emphasized the development of "formal theories" of organizations. (See, for example, Peter M. Blau, "A Formal Theory of Differentiation in Organizations," *American Sociological Review*, Vol. 35, 1970.)

Also of interest are investigations of mass communication, propaganda, and persuasion. Mathematical models of message diffusion have been developed in the context of air-dropped leaflets. (Melvin L. DeFleur and Otto N. Larsen, *The Flow of Information*, New York, Harper, 1958.)

Ethnic and race relations has been a field attracting much interest. William I. Thomas and Florian Znaniecki's *The Polish Peasant in Europe and America* (Chicago, University of Chicago Press, 1918) was an early landmark study. Another influential book was Gunnar Myrdal's *An American Dilemma* (New York, Harper, 1944). For a sample of recent work, see Seymour Spilerman, "The Causes of Racial Disturbances: A Comparison of Alternative Explanations" (*American Sociological Review*, Vol. 35, 1970.)

Many inquiries have been made into the religious, legal, educational, political, economic, military, and other institutions of contemporary society. In addition, the relations of man's intellectual efforts to the social setting have been investigated in fields such as the sociology of science and the sociology of knowledge, subject matters that now seem to be attracting increasing attention. (See, for example, Hans Neisser, *On the Sociology of Knowledge*, New York, Heineman, 1965; Peter L. Berger and Thomas Luckman, *The Social Construction of Reality*, Garden City, Doubleday, 1966; and Gunter Remmling, *Road to Suspicion*, New York, Appleton-Century-Crofts, 1967.)

Inquiries into social stratification investigate the ways in which people are ranked according to some characteristic regarded as important, such as wealth or influence; the differential distribution of rewards; the effects on behavior patterns of such differentiations; the relation of stratification to social mobility, stability, conflict, and change; and related matters. Connections are found between social stratification and a very large number of behaviors, e.g., patterns of crime, divorce, recreation, and religion. (See Richard H. Hall, *Occupations and the Social Structure*, Englewood Cliffs, Prentice-Hall, 1969; Jack L. Roach, Llewellyn Gross, and Orville R. Gurslin, eds., *Social Stratification in the United States*, Englewood Cliffs, Prentice-Hall, 1969; and William A. Rushing, "Two Patterns in the Relationship between Social Class and Mental Hospitalization," *American Sociological Review*, Vol. 34, 1969.)

Social Deviance and Social Problems

Many early American sociologists were involved in social reform movements and hoped to find ways of ameliorating social ills. As the field developed, considerable emphasis continued to be placed on problems such as crime, prostitution, alcoholism, drug addiction, etc. Deviant behavior often is investigated in terms of the development and enforcement of social rules, the circumstances and conditions under which those roles will probably be challenged, the study of subgroups condoning or approving deviant behavior, and the relations of deviant behavior and the measures by which the larger groups try to control it. Currently attention is also given to collective deviant behavior, as is found in youth "counter-cultures." In inquiry into social disorganization, the focus is not on the individual's deviant behavior, but on the breakdown of institutional processes and the loss or decreased acceptance of group codes of behavior, attitudes, etc.

(See Albert K. Cohen, *Delinquent Boys: The Culture of the Gang*, Glencoe, Free Press, 1955; Robert K. Merton and Robert A. Nisbet, eds., *Contemporary Social Problems*, New York, Harcourt, Brace, 1961; F. James Davis, *Social Problems*, New York, Free Press, 1970; and Simon Dinitz, Russell R. Dynes, and Alfred C. Clarke, eds., *Deviance: Studies in the Process of Stigmatization and Societal Reaction*, New York, Oxford University Press, 1969.)

Social Change

Considerable sociological effort also has gone into inquiry into the processes of social change and the problems associated with those changes. Various ways in which change occurs are studied, such as assimilation, diffusion, innovation, competition, conflict, etc. William F. Ogburn was among the first of modern sociologists to attempt a systematic analysis of social change; he emphasized the notion of "cultural lag." More recent inquiries focus on large scale institutional changes that occur when a society is modernized; on social movements such as reform and radical groups, religious cults, new political parties, etc.; and on historically oriented descriptions of major social changes.

(See, for example, Robert A. Nisbet, *Social Change and History*, London, Oxford University Press, 1969; Kai T. Erikson, *Wayward Puritans*, New York, Wiley, 1966; W.G. Runciman, *Relative Deprivation and Social Justice*, Berkeley, University of California Press, 1966; and Werner J. Cahnman and Alvin Boskoff, eds., *Sociology and History*, New York, Free Press, 1964.)

* * * * * * *

The foregoing survey of the range of sociological literature includes results ranging from warranted assertions about human behavior to what the investigators regard as highly plausible statements about social phenomena. As an example of the former, John A. Clausen reports on predictions made on the basis of questionnaire and attitude scales of what fields of endeavor would be entered by veterans returning to civilian life. The predictions made generally proved to be reliable and accurate. ("Studies of the Post War Plans of Soldiers: A Problem in Prediction," in Samuel Stouffer *et. al.*, eds., *Measurement and Prediction*, Vol. IV of *Studies in Social Psychology in World War II*, Princeton, Princeton University Press, 1950, Chs. 15, 16.) Some sociologists regard such results as relatively trivial and believe "explaining" and "understanding" are more important than prediction, which returns us to the topic of controversies among sociologists.

5. CONTEMPORARY CONTROVERSY

An interesting development already noted is the re-emergence of controversies that formerly generated much discussion but which seemed to have abated. The main thrust of recent criticisms of the natural science trend is little different from the criticisms made in the 1930's and 1940's. In that period George Lundberg and Read Bain (among others) replied in detail to nearly all the arguments currently found that human behavior is best studied in some nonscientific way. (Lundberg, *Foundations of Sociology*, New York, Macmillan, 1939, and *Can Science Save Us?*, New York, Longmans, Green, 1947, 2nd ed., 1961; Bain, "Sociology as a Natural Science," *American Journal of Sociology*, Vol. LIII, 1947.)

Perhaps the most important current controversies concern methodological issues. Those issues often overlap; for convenience they will be discussed under several headings.

Theorizing and Observation of Data

Some sociologists have attempted to concentrate almost exclusively on the collection of data without using explicit conjectures or hypotheses. Such "descriptive empiricists" have been criticized on the grounds that implicit, unrecognized "theory" may bias the results, that their findings are random, petty, and helter-skelter, that the mere collection of facts is only part of scientific inquiry, etc. (See Robert Bierstedt, "A Critique of Empiricism in Sociology," *American Sociological Review*, Vol. 14, 1949; McKinney, 1957, *op. cit.*)

There are also sociologists who regard conclusions based on experiments in laboratory situations as unlikely to apply to "natural" situations because of the artificiality of laboratory settings. Sometimes such critics urge that natural social behavior be investigated by a phenomenological or participant observer method (for example, see Theodore Mills, *op. cit.*)

Other sociologists, those whom C. Wright Mills called the "grand theorists," developed obscure notions that were either untestable in principle or at least beyond our present capability of testing. (*The Sociological Imagination*, New York, Oxford University Press, 1959.)

Arthur L. Stinchcombe, while not defending "grand theories," argues that "a sociologist is ordinarily confronted with phenomena for which there are no theories," and that under contemporary conditions it is more important for sociologists to invent than to test theories. (*Constructing Social Theories*, New York, Harcourt, Brace and World, 1968.) And Barney Glaser and Anselm Strauss, although urging that theory be derived from a close familiarity with data, also separate sharply the "generation" and the "verification" of theory. They maintain that too much attention has gone into testing, and regret that too often young sociologists are taught that they are not functioning as sociologists unless they are involved in verifying their "theories." (*The Discovery of Grounded Theory*, Chicago, Aldine, 1967.)

Some sociologists emphasized the development of "middle range theories" that are testable but not trivial. (Thomas K. Marshall, *Sociology at the Crossroads*, London, Longmans, Green, 1947.) Robert K. Merton believed that middle range theories were the most productive and argued (in 1949) that theoretical and empirical work were being united in sociological inquiry:

> "The stereotype of the social theorist high in the empyrean of pure ideas uncontaminated by mundane facts is fast becoming no less outmoded than the stereotype of the social researcher equipped with questionnaire and pencil hot on the chase of the isolated and meaningless statistic. For in building the mansion of sociology during the last decades, theorist and empiricist have learned to work together. What is more, they have learned to talk to one another in the process." (Merton, *op. cit.*, p. 97.)

The recent literary, voluntaristic, and subjectivist procedures of inquiry being emphasized may lead to an even greater split between the development of conjectures (hypotheses) and the collection of data than that characterizing earlier sociological inquiry. (For an account of what we believe is the most useful relation between conjectures and data, see the closing section of Ch. 1 on the course of inquiry.)

Priority of Problems

One group of sociologists has advocated work on important human problems for which techniques of desirable rigor are not yet available, much along the lines argued by Robert S. Lynd (*Knowledge for What?*, Princeton, Princeton University Press, 1939). Other sociologists emphasized the difficulties that can result from such attempts and advocated concentrating on problems that can be handled with the methods now available. C. Wright Mills was strongly critical of the members of the latter group, whom he called "abstracted empiricists." Mills held that they tended to select their problems solely on the basis of methodological criteria, rather than on the basis of human significance. (C. Wright Mills, *op. cit.*) Some of the natural science sociologists (perhaps Lundberg most prominently) advocated an attack on the urgent problems of men-in-society, but by the use of "hard" methods and with the recognition that at any given time some urgent problems cannot be satisfactorily solved because we simply do not have the required information to do so. (Lundberg, 1947 & 1961, *op. cit.*)

In the last few years, some sociologists have not only urged that the "burning issues" be treated, but maintain that conventional sociologists cannot do so because they only function in trivial ways or as rationalizing agents for the group holding power in our society. A radical sociology is called for in which sociologists side with the down-trodden, the dispossessed, and the exploited. (See Steven E. Deutsch and John Howard, eds., *Where It's At: Radical Perspectives in Sociology*, New York, Harper and Row, 1970; Gouldner, *op. cit.;* J. David Colfax and Jack L. Roach, eds., *Radical Sociology*, New York, Basic Books, 1971; and Norman Birnbaum, *Toward a Critical Sociology*, New York, Oxford, 1971.)

Operationism

In many behavioral science areas operationism and related issues have generated much controversy, even though often what "operationism" names is not clear. (See Chapter on Psychology.) Operationists often regard their procedures as highly useful in guarding against subjectivity and untestability. Their opponents often claim that operationism unduly narrows the range of inquiry, that the "essential meaning" of terms is missed, or that many useful scientific terms cannot be given a direct operational interpretation.

By the middle 1950's the controversies about operationism had diminished among sociologists. In 1957, for example, McKinney believed that "instrumentalism" (not to be confused in this context with an adulation of scientific equipment) was a variety of operationism that was accepted by most sociologists:

> "Originally formulated by Dewey, it is now a prevailing orientation of both the theoretically and the empirically inclined. Instrumentalism (often still

> traveling under the label of operationism) simply maintained that concepts should be made subject to inquiry and susceptible to hypothetical statement for purposes of examination. Moreover, instrumentalism asserts that theories, discrete or systematic, must be evaluated in terms of their research adaptability, verifiability, and fruitfulness." (McKinney, 1957, *op. cit.*, pp. 208-209.)

More recently, the procedures of inquiry Dewey defended as useful have been challenged by many sociologists, and one cannot say that contemporary sociologists by and large accept those procedures.

Sociology as Normative

Many sociologists have argued that scientific inquiry is "value free" in the sense of "morally neutral." Lundberg, for example, said:

> "As *science*, both physical and social sciences have a common function, namely, to answer scientific questions. These answers will always be of an impersonal, conditional type: '*If* the temperature falls to 32°F., *then* water (H_2O) will freeze.' '*If* a certain type of tax is adopted, *then* certain types of industrial activity will decrease.' Neither of these statements carries any implications as to whether or how the knowledge should be used. Far from being a weakness, this characteristic of scientific knowledge is its greatest strength. . . . [T]hose scientists and others who try to identify science with some particular social program, sect, or party must be regarded as the most dangerous enemies of science. They are more dangerous than its avowed enemies, because the defenders of 'democratic,' 'communist,' 'religious,' or 'moral' science pose as defenders of science and carry on their agitation in the name of lofty social sentiments. . . . Unfortunately, the same has been said for prominent proponents of the Inquisition." (Lundberg, 1961, *op. cit.*, pp. 40-41. He makes the same point in *Foundations of Sociology*, revised and abridged ed., New York, David McKay, 1964, pp. 24-35.)

That view has come under strong attack from many writers. Irving L. Horowitz, for example, associates value neutrality with moral aloofness and says: "The truth of course is not that values have actually disappeared from the social sciences, rather that the social scientist has become so identified with the going value-system." (Horowitz, *op. cit.*, p. 10.) Richard L. Means argues, as have others before him, that sociologists must adopt ethical positions if they are to study a society in which ethical issues are central. (*The Ethical Imperative*, Garden City, Doubleday, 1969.) A.R. Louch maintains that the social sciences are necessarily moral and that therefore social scientists must openly adopt a moral perspective. (*Explanation and Human Action*, Berkeley, University of California Press, 1967.) Howard S. Becker emphasizes the influences of values on "objective" work and mentions that every inquiry is made from a point of view. ("Whose Side Are We On?," *Social Problems*, Vol. 14, 1967.)

Many other works on similar themes could be cited. In addition, there are discussions of the possible moral damage research may inflict on the subjects by tampering with their lives, belief systems, etc. (John R. Seeley, *The Americanization of the Unconscious*, New York, International Science Press, 1967), and discussions of unrecognized ideological influences on sociological conjectures (I. M. Zeitlin, *Ideology and the Development of Sociological Theory*, Englewood Cliffs, Prentice-Hall, 1968.)

Similar issues have been heatedly discussed throughout much of the history of sociology. Underlying many of the issues is a dispute as to whether the objective of sociologists is the development of warranted assertions that describe what happens under specified circumstances, or is the development of a moral, political, religious, literary, etc., standpoint on human affairs that will lead to "good" action. To illustrate, in the editorial foreword to the journal *Sociologia Internationalis*, one finds the following:

> "At present two major schools of thought. . .are confronting each other. One is the systematic or verstehende type of sociology. . . . The other is the practical, the empirical or the observational type. . . . Only when the synthesis of both these methods comes together on the highest level of human action can we proceed in the fundamental task of sociology which may be called *Lebenskunst* or 'art of living.' Lebenskunst must be based upon both truthful thinking and correct action."

This type of issue apparently is deeply enmeshed in sociological thought. The Smelser-Davis report on the state of sociology says:

> ". . .sociology overlaps with religious, moral, and political doctrines because all involve general assertions about man's relation to man. A consequence of this is that much of what is called 'theoretical analysis' in sociology is, in fact, an effort to relate the work of a sociological theorist to some epistemological, moral, or political position. . . . In any case, given sociology's kinship with political and moral preoccupations, it is reasonable to expect that scientific sociology will not soon be separated from man's propensity to politicize and moralize." (Smelser and Davis, *op. cit.*, p. 36.)

6. TERMINOLOGICAL PROBLEMS

As in any field, there are difficulties with the more technical and specialized terms used in sociological inquiry. In our opinion, however, of far greater importance are the problems concerning key names bearing on the methods used and the subject matters chosen for investigation. Some problems of that type will be discussed here.

John Dewey and Arthur F. Bentley described three types of procedure for inquiry, the *self-actional*, the *interactional*, and the *transactional*, and they analyzed with care the defects in the first two as used in inquiry into human behavior. (*Knowing and the Known*, Boston, Beacon Press, 1949; reprinted in its entirety in Rollo Handy and E.C. Harwood, *Useful Procedures of Inquiry*, Great Barrington, Behavioral Research Council, 1973.) All three procedures can be found in contemporary sociological inquiry.

Self-actional assumptions long have been abandoned in most scientific fields; the attribution of events to the acts of independent entities, powers, or selves, as in "explaining" thunderbolts as the result of Zeus' anger, has not been useful. But some sociologists use self-actional procedures of inquiry; many symbolic interactionists place considerable emphasis on self-actional notions and terminology, as illustrated in the statement that at "the heart of group life lies a series of social selves that have been lodged in that structure." (Norman K. Denzin, "Symbolic Interactionism and Ethnomethodology: A Proposed Synthesis," *American Sociological Review*, Vol. 34, 1969, p. 923.) Other sociologists also stress the acts of actors, sometimes in a way suggesting entirely free actions, and at other times using an interactional procedure in which putatively independent actors, selves, or persons are brought into some kind of causal relation with other actors.

The extremely heavy use of "meaning" among some sociologists also gives rise to problems. Duncan, for example, after saying that "it is impossible to talk about human relationships without saying *something* about meaning," goes on to conclude:

> "So, unless we assume that action is 'patterned' by some extrasymbolic 'force' or is 'determined' by a past, or a future, or is simply a random activity, we must assume that action is determined by the form in which men communicate as they act together, and that the creation of such forms is, therefore, the creation of ways in which we relate in society." (Duncan, *op. cit.*, p. 5, p. 48.)

The widespread use of mentalistic language among the symbolic interactionists is interesting in that they often trace the origin of their procedures of inquiry from George H. Mead through John Dewey, but apparently without being aware of the criticisms Dewey made of "meaning," "self," "concept," "definition," and related names. Many of the symbolic interactionists apparently are groping for procedures something like those Dewey and Bentley used in their inquiry into sign-behavior, but they rely on terminology that Dewey and Bentley demonstrated was worse than useless for inquiring into behavioral transactions.

"Interaction" is widely used as a key name by sociologists of many points of view. Thomas P. Wilson, for example, says that sociological inquiry fundamentally "depends upon descriptions of interactions," and that "large-scale social phenomena" can be viewed as "patterned relations among the actions of individual actors in interaction with one another." ("Conceptions of Interaction and Forms of Sociological Explanation," *American Sociological Review*, Vol. 35, 1970, p. 704, p. 698.) Sometimes "interaction" is used in a way suggesting that separate "reals" causally affect each other within some system, as in a reaction away from, or an action toward, something else. At other times, it is used as "transaction" is used in Chapter I, to name mutual, reciprocally influenced, connections within a field of activity. Inconsistency and incoherence frequently are found, especially in attempts to specify what the interacting units are.

The current controversies about value neutrality in behavioral inquiry are intermeshed with conflicting and ambiguous uses of "value," which variously names preferences, convictions, goals, criteria, approvals, desires, notions of the desirable as contrasted to the desired, etc.

7. COMMENT AND EVALUATION

The extent to which many recent sociologists have rejected the so-called natural science procedures of inquiry has been noted repeatedly in this Chapter. Many other sociologists, on the other hand, do emphasize scientific procedures, and possibly a renewed interest in scientific sociology will be seen in the future. (See, for example, Peter Park's call for scientific procedures in his *Sociology Tomorrow: An Evaluation of Sociological Theories in Terms of Science*, New York, Pegasus, 1969.)

Perhaps much of the dissatisfaction with scientific procedures of inquiry occurred because many of the results obtained were fragmentary and lacked wide application. George Murdock, in a comparison of anthropology and sociology, says:

> "[Sociology] gives the impression, if one judges by the periodical literature rather than the tomes of the system-builders, of being composed of a very large number of fragmentary and isolated propositions each tested and at least tentatively validated, which are mainly of a relatively low order of generality. . . ." ("Sociology and Anthropology," in John Gillin, ed., *For a Science of Social Man*, New York, Macmillan, 1954, p. 24.)

The remedy for such fragmentary warranted assertions, according to many sociologists, is the further development of integrating theory. Recently, for example, Lee Freese has complained that sociological inquiry has not resulted in "cumulative knowledge" of the type found in the physical sciences. He suggests that even the development of "tested theories" will not suffice, unless such theories have a "common theoretical denominator," which will not be possible "without applying formal rules for systematizing verbal propositions." ("Cumulative Sociological Knowledge," *American Sociological Review*, Vol. 37, 1972, pp. 472-473, 481, 482.)

But past attempts to develop integrating theory (often formalized), in advance of testing, has not led to useful results, and has been followed by many criticisms that such work was only pseudo-scientific. The door was then opened to the use of allegedly superior nonscientific procedures. We suggest that probably a major reason useful solutions to the "burning issues" have not been found is that the relevant sign-behavior has been investigated either by using inappropriate scientific procedures copied from physical and physiological inquiries, or by using traditional mentalistic and subjectivistic procedures.

Many sociological writings do not appear to reflect a high level of familiarity with the development of modern scientific inquiry. In 1955 Lundberg found it lamentable that "a field of study as prominent as sociology. . .and making such vigorous claims to the status of science, should have paid so little attention to a formulation of its basic postulates, its logic, and its methods." (Lundberg, 1955, *op. cit.*, p. 197.) And many years later we still find sociologists relying on traditional epistemological formulations of what is involved. David Willer and Judith Willer, for example, say: "Scientific knowledge consists of a combination of empirical and theoretical knowledge. The former is the result of research or the *collection of sense data*, while the latter is concerned with the *mental relations of nonobservable constructs*." ("Why Sociological Knowledge is Not Cumulative: A Reply to Professor Freese," *American Sociological Review*, Vol. 37, 1972, p. 483. Italics added.)

Some comments in the Smelser-Davis report also may be pertinent. Relying on Elbridge Sibley's survey (*The Education of Sociologists in the United States*, New York, Russell Sage Foundation, 1963), they note that many

PhD's in sociology believed that their doctoral education had been deficient "in research training, mathematics and statistics, training in related disciplines, and training in theory, philosophy, and logic." They also say:

> "Sociology emerges as a discipline whose potential recruits are exposed to its *professional and social-scientific aspects* less and later than in other behavioral and social sciences. . . . With respect to sociology, women entering the field are somewhat less able than women entering other graduate fields; and men planning graduate study in sociology are not only less able than men in other behavioral and social sciences, but also below the calibre of graduating seniors in general." (Smelser and Davis, *op. cit.*, pp. 141-142, p. 136.)

In our opinion, the problems sociologists are grappling with are most usefully investigated by transactional procedures of inquiry, a method used implicitly by some sociologists and more explicitly by those "interactionists" who emphasize the mutual, reciprocal relations in what they call interactions. Some of the controversies about special methods also can be resolved in the transactional framework.

For example, the emphasis functionalists place on the notions of adjustive behavior, the interdependence of the parts of a system, and dynamic unity, fits nicely within transactional procedures, and there appears to be no need to regard functionalism as a special form or method of description that is peculiarly appropriate to behavioral inquiry as contrasted to physical inquiry. And the symbolic interactionists' emphasis on the importance of communication and "meaning" in describing human behavior can, we believe, be more usefully handled by inquiring into sign-behavior transactions without reifying selves, meanings, etc.

Similarly, once it is recognized that in behavioral inquiry the inquirer is in common process with what is being inquired into, participation in what is being observed is neither to be deplored (as some critics of participant observation claim) nor to be acclaimed as a method superior to ordinary scientific observation (as some supporters of participant observation maintain). As Arthur F. Bentley noted as long ago as 1935, in the typical subject matters sociologists investigate the observer is necessarily a participant:

> "We must face the condition that we, the investigators, are participants in what we investigate; that our participation is 'local' within it. . . ; that the definite determination of such localizations, however difficult, is essential to the interpretation of what we, thus localized, observe;. . .and that the two-fold construction of the observation, in terms, on the one side, of what is observed, and, on the other side, of the position from which the observation is made, is essential to any dependable knowledge of the kind we call scientific." (*Behavior, Knowledge, Fact*, Bloomington, Principia Press, 1935, p. 381.)

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9. GERMANE JOURNALS

ADMINISTRATIVE SCIENCE QUARTERLY
AMERICAN JOURNAL OF ECONOMICS AND SOCIOLOGY
AMERICAN JOURNAL OF SOCIOLOGY
AMERICAN SOCIOLOGICAL REVIEW
ANNALS OF THE AMERICAN ACADEMY OF POLITICAL AND SOCIAL SCIENCE
BRITISH JOURNAL OF SOCIOLOGY
COMPARATIVE GROUP STUDIES
COMPARATIVE STUDIES IN SOCIETY AND HISTORY
CONTEMPORARY SOCIOLOGY: A JOURNAL OF REVIEWS
CRIMINOLOGY
CURRENT SOCIOLOGY
EUROPEAN JOURNAL OF SOCIOLOGY
HUMAN RELATIONS
IMPACT OF SCIENCE ON SOCIETY
INTERNATIONAL SOCIAL SCIENCE JOURNAL
JOURNAL OF CRIMINAL LAW, CRIMINOLOGY AND POLICE SCIENCE
JOURNAL OF MARRIAGE AND THE FAMILY
JOURNAL OF SOCIAL ISSUES

PACIFIC SOCIOLOGICAL REVIEW
POPULATION INDEX
PUBLIC OPINION QUARTERLY
RURAL SOCIOLOGY
SCHOOL AND SOCIETY
SCIENCE AND SOCIETY
SOCIAL FORCES
SOCIAL PROBLEMS
SOCIAL RESEARCH
SOCIAL SCIENCE RESEARCH
SOCIETY
SOCIOLOGIA INTERNATIONALIS
SOCIOLOGICAL ABSTRACTS
SOCIOLOGICAL INQUIRY
SOCIOLOGICAL METHODS AND RESEARCH
SOCIOLOGY AND SOCIAL RESEARCH
SOCIOLOGY OF EDUCATION
SOCIOMETRY

V.

POLITICAL SCIENCE*

1. WORKING DESCRIPTION OF THE FIELD

POLITICAL scientists inquire into the behavior of human individuals and groups, with an emphasis on the distribution and attainment of political influence and power, and the functioning, organization, and connections among the political units, the institutions, the laws, and the customs by means of which humans are governed.

Political science inquiries overlap inquiries in many other behavioral fields, including sociology, psychology, economics, anthropology, history, and jurisprudence.

2. OTHER DESCRIPTIONS OF THE FIELD

Political inquiry has had a long history and not surprisingly many different viewpoints have developed as to what the field is or should be. For some time, the emphasis was on the *state*, often interpreted according to some metaphysical system.

In recent years, the descriptions offered of the field frequently emphasize inquiry into governmental processes, into power, and influence. For example, according to the Report by The Behavioral and Social Sciences Survey Committee (sponsored by the National Academy of Sciences and the Social Science Research Council):

> "Political science is concerned with government in all its aspects, in both theory and practice: political parties, interest groups, public opinion and communication, bureaucracy, administration, and international relations." (*The Behavioral and Social Sciences: Outlook and Needs*, Englewood Cliffs, Prentice-Hall, 1969, p. 38.)

Peter Odegard says:

> "The political scientists' special preoccupation is with those aspects of human behavior having to do with the exercise of political power as a means of social control. . . . It is therefore with the nature, basis, structure, scope, and dynamics of political power that the political scientist is mainly, although not entirely, concerned." ("Politics: A New Look at Leviathan," in Lynn White, Jr., ed., *Frontiers of Knowledge*, New York, Harper, 1956, p. 96.)

And V.O. Key says:

> "Broadly, the study of politics is concerned with the institutions and processes of governance. The study of the structure of government, of constitutions, of administration, of international relations, of the legislation process, of the dynamics of the political struggle, all revolve around the problem of the governing of men. . . . A single thread runs through all the areas into which political scientists divide their subject for study. That thread is power." (*Politics, Parties, and Pressure Groups*, 2nd ed., New York, Crowell, 1947, pp. 1-2.)

Others, such as George E.G. Catlin, emphasize the general notion of *social control:*

> "We can call politics, if we like (along with Professor V.O. Key), 'the study of government,' provided that we bear in mind that 'government' here must be used as a synonym for 'control,' and carries no necessary implication of presidents and cabinets; that our theory has to cover the arguments of anarchism; and that we must beware of the fallacy of loaded terms and of packing into our definition an authoritarianism at the beginning which we hope, as I have said, to pull out of the hat at the end. We can also call it 'the study of power and influence' if, with George Washington, we bear in mind that 'influence is not government' . . . . The field of political science is *the field of study of social controls*, or, more specifically, of the control relationship of human, and even animal, wills. . . . The unit of political science is the individual act of control." ("Political Theory: What Is It?," *Political Science Quarterly*, Vol. LXXII, 1957, pp. 6-10.)

Sometimes *influence* is emphasized. Harold Lasswell, for example, distinguishes between the "science of politics," which "states conditions," and the "philosophy of politics," which "justifies preferences," and says: "The study of politics is the study of influence and the influential. . . . The influential are those who get the most of what there is to get. Available values may be classified as *deference, income, safety.*" (*Politics: Who Gets What, When, How*, New York, McGraw-Hill, 1936, p. 13, p. 187.)

David Easton reviewed the many different tasks and objectives exhibited in political science literature to see if they had anything in common around which a comprehensive framework could be developed for ordering institutional data as well as data about individual behavior. He concluded that all political scientists share a concern with the same basic problem, which he described as "the authoritative allocation of values for a society." (*The Political System*, New York, Knopf, 1953, p. 129.)

Many political scientists describe their field broadly enough to include what ordinarily may not be regarded as political. Robert A. Dahl, for example, says: "A political system is any persistent pattern of human relationships that involves, to a significant extent, power, rule, or authority," and thus not only businesses, religious organizations, private clubs, etc., but "perhaps even families" have political systems. (*Modern Political Analysis*, Englewood Cliffs, Prentice-Hall, 1963, p. 6.) Charles Merriam suggested that political inquiry is directed toward the structure of organizations in general, not only toward governmental organizations, and other political scientists also have followed his lead. (*Public and Private Government*, New Haven, Yale University Press, 1944.)

In recent years there has been an emphasis on inquiry into political events viewed as interactional processes. Felix E. Oppenheim, for example, says:

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*Samuel Krislov critically analyzed an earlier version of this chapter. We appreciate his help, but emphasize that he is not responsible for the contents of this chapter.

> "Social science in general has become the science of human interaction, and political science in particular, that of political interaction. Key concepts such as influence, control, power, authority are now interpreted as relationships of interaction among persons or groups." (*Dimensions of Freedom*, New York, St. Martin's Press, 1961, p. 4.)

In the 1950's and 1960's the *behavioralists* became an influential group within political science. They maintained that "the science of politics is a science of behavior." (James G. Marsh, "An Introduction to the Theory and Measurement of Influence," *American Political Science Review*, Vol. 49, 1955, p. 431.) Beginning as a protest against the nonscientific procedures of inquiry then prevalent, the "political behavior movement" now often is regarded as a major, if not the dominant, trend in the field.

Some political scientists have emphasized applying the findings of political inquiry to administrative and other relevant needs of the time, as in Lasswell's call for a "policy orientation." ("The Policy Orientation," in Daniel Lerner and Harold D. Lasswell, eds., *The Policy Sciences*, Stanford, Stanford University Press, 1951.) Some policy science advocates maintain that in addition to describing political processes, political scientists can also help to *prescribe* what should be done.

3. METHODS AND TYPES OF INQUIRY

In this Section some of the major emphases in the procedures of inquiry used by political scientists are discussed. Those emphases are not necessarily to be identified with schools of thought, and there can be overlapping among the themes, which were chosen simply for convenience.

Philosophical Methods

The recent emphasis on political inquiry as scientific has been criticized by numerous authors. Some critics are still concerned primarily with the history of ideas. Some consider politics as a practical art, not a science. Others regard it as a branch of history that describes unique or particular events in political history. Some pursue the quest for a satisfying ideology. Some argue that political philosophy must raise "basic" questions about the "nature of the state" and the "nature of justice."

Leo Strauss and his followers criticize present-day scientifically oriented inquirers for rejecting the aims of classical political philosophy and for being too much absorbed in method. Classical political philosophy, Strauss claims, was essentially "practical" and was concerned with normative questions such as the nature of virtue and the best political order. Strauss maintains that political philosophy is an essential part of the study of politics, and that political scientists who believe their inquiries can be value-free or ethically neutral are mistaken. *Essays on the Scientific Study of Politics*, edited by H.J. Storing (New York, Holt, Rinehart & Winston, 1962), reflects a Straussian method and attacks the various notions of scientific inquiry held by A.F. Bentley, Harold Lasswell, and Herbert A. Simon.

Eric Voegelin's work is an extreme instance of the revolt against "positivism," "relativism," and scientific inquiry. His many-volumed *Order and History* attempts a comprehensive synthesis in the grand style: "Every society is burdened with the task, under its concrete conditions, of creating an order that will endow the fact of its existence with meaning in terms of ends divine and human." (*Order and History*, Vol. I, Baton Rouge, Louisiana State University Press, 1956, p. ix.)

Others, such as Herbert Marcuse, have offered a more ideologically and less metaphysically oriented philosophical critique of contemporary society. (*One Dimensional Man*, Boston, Beacon Press, 1964; and, with Robert Paul Wolff and Barrington Moore, *A Critique of Pure Tolerance*, Boston, Beacon Press, 1965.) Mulford Q. Sibley adopts a utopian approach in his recent book; his work is a link also to "natural law" views. (*Political Ideas and Ideologies*, New York, Harper and Row, 1970.)

Procedures Emphasizing Theories and Models

Along with workers in other fields, political scientists use "theory" in diverse ways. Confusion may result from an oscillation between "theory" as applied to highly warranted scientific findings, as in "theory of evolution," and "theory" as applied to untested–or only partially tested–conjectures of broad range, as in "psychoanalytic theory of politics." (One suspects that sometimes the oscillation is only too convenient.)

Many political scientists, although sympathetic to scientific inquiry and wary of metaphysics, place a strong emphasis on theorizing. Arnold Brecht, for example, argues that although political science should avoid any simple dependence upon "the history of ideas," theory should be emphasized. (*Political Theory: The Foundations of Twentieth-Century Political Thought*, Princeton, Princeton University Press, 1959.)

David Easton also advocated that point of view and complained that political science has lagged behind sociology and economics in achieving a general or systematic theory. Easton wants political scientists to develop a great integrating theory like Einstein's in physics or Darwin's in biology. He urges a comprehensive theoretical scheme that will guide, measure, and stimulate research, along the lines of Talcott Parsons' work in sociology. He advocates a system of working hypotheses that is "adopted and used only as long as it helps orient empirical research in such a way that socially significant problems are better understood." His general system would consist of postulates from which narrower generalizations would be deduced. From these, in turn, specific generalizations "capable of empirical proof" would be deduced. (Easton, *op. cit.*, pp. 57-58.) In his later presidential address (*American Political Science Review*, Vol. LXIII, 1969), Easton calls for behavioral precision in dealing with important policy questions and the synthesizing of some of the conflicting tendencies in political science.

Harold Lasswell and Abraham Kaplan attempted to construct a "conceptual framework" for political inquiry that would advance political theory, and insisted that "theorizing, even about politics, is not to be confused with metaphysical speculation in terms of abstractions hopelessly removed from empirical observation and control." (*Power and Society: A Framework for Political Inquiry*, New Haven, Yale University Press, 1950, p. x.) Like workers in many other fields, they apparently believed that elaborate conjectures can usefully be developed in advance of testing. Their general aim was:

> ". . .to elaborate a conceptual framework within which inquiry into the political process may fruitfully proceed. For, at bottom, it is only on the

> basis of such inquiry that political policy can be intelligently selected and applied. . . . Our purpose is primarily to advance political theory. . . . In these terms, the present work is an attempt to formulate the basic concepts and hypotheses of political science." (*Ibid.*, pp. x-xi.)

Others have attempted to develop useful models for political inquiry from game and decision theory. For example, R. Duncan Luce and Arnold A. Rogow argue that mathematical models are useful in political science, with the "theory of games providing a model for conflicts between intelligent and goal-seeking agents," and they outline a possible application to the congressional power structure. ("A Game Theoretic Analysis of Congressional Power Distributions for a Stable Two-Party System," *Behavioral Science*, Vol. I, 1956.) Another example is L.S. Shapley and M. Shubik's endeavor to apply game theory techniques to concrete problems such as the distribution of power in a committee system. ("A Method for Evaluating the Distribution of Power in a Committee System," *American Political Science Review*, Vol. 48, 1954.)

William H. Riker and William J. Zavoina review some of the disputes about "rational behavior" and "utilities" and believe they have found at least indirect evidence that in situations where the participants can make choices, "utility maximization is the theory that fits political behavior best." ("Rational Behavior in Politics: Evidence from a Three Person Game," *American Political Science Review*, Vol. 64, 1970, p. 60.)

Riker has been one of the major exponents of analytical mathematical models. (*Theory of Political Coalitions*, New Haven, Yale University Press, 1962.) Gordon Tulloch has emphasized logical model building and the use of economic models in political inquiry. (*Toward a Mathematics of Politics*, Ann Arbor, University of Michigan Press, 1967.) Other mathematical procedures are also found, as in, for example, Douglas Rae and Michael Taylor's *The Analysis of Political Cleavages* (New Haven, Yale University Press, 1970), and Brian Barry's *Political Argument* (London, Routledge and Kegan Paul, 1965).

Comparative Method

There is a long tradition in political science, going back to the classical Greeks, of comparing aspects of differing political systems. Often the "comparative method" was viewed as a separate or special method, although all behavioral inquiry involves comparisons, and comparisons of political phenomena may use a variety of scientific and unscientific procedures. According to Roy M. Macridis, contemporary comparative inquiry proceeds as follows:

> "(1) the collection and description of facts on the basis of carefully constructed and generally adhered to classificatory schemes; (2) the discovery and description of uniformities and differences; (3) the formulations of interrelationships between component elements of the political process and other social phenomena in the form of tentative hypotheses; (4) the subsequent verification of such tentative hypotheses by rigorous empirical observation for the purpose of amplifying the original hypotheses and ultimately verifying them; and finally, (5) the slow, cumulative process of 'acceptance' of certain basic propositions." (*The Study of Comparative Government*, New York, Random House, 1955, p. 4.)

Three types of comparison are frequently found: (1) the *configurational*, in which comparisons are made of entire political systems; (2) the *institutional*, in which the structures of particular institutions are compared (e.g., judicial systems); and (3) the *functional*, in which the various operations of a system or institution are compared (e.g., the methods of judicial review).

Comparative studies, as thus viewed, do not use a "special" method, but rather use regular investigative techniques of classification and comparison to develop and test conjectures or hypotheses about possible connections.

The work of Gabriel Almond and the Social Science Research Council's Committee on Comparative Politics may also be mentioned. Almond has emphasized evolutionary models for societies, as opposed to a single model, and the need for the clarification of key processes. (Gabriel Almond and J.S. Coleman, eds., *The Politics of the Developing Areas*, Princeton, Princeton University Press, 1960; Gabriel Almond and G. Powell Bingham, *Comparative Politics: A Developmental Approach*, Boston, Little, Brown, 1966.)

Behavioralism

A group of contemporary political scientists, usually called *behavioralists*, have adopted scientific procedures of inquiry. Broadly speaking, they continue some of the earlier scientific emphases as found in Graham Wallas (*Human Nature in Politics*, New York, Appleton-Century-Crofts, 1908) and Charles E. Merriam (*New Aspects of Politics*, Chicago, University of Chicago Press, 1925). A major influence was that of Arthur F. Bentley's now famous but long neglected work of 1908, *The Process of Government: A Study of Social Pressures*. Bentley said:

> "[W]e have a dead political science. It is a formal study of the most external characteristics of governing institutions. It loves to classify governments by incidental attributes, and when all is said and done it cannot classify them much better now than by lifting up bodily Aristotle's monarchies, aristocracies, and democracies which he found significant for Greek institutions, and using them for measurements of all sorts and conditions of modern governments." (Bentley, *op. cit.*, 1935 ed., Bloomington, Principia Press, p. 162.)

Bentley maintained that to describe governmental processes as they operate, one must inquire into the group interests at work: "All phenomena of government are phenomena of groups pressing one another, forming one another, and pushing out new groups and group representatives (the organs or agencies of government) to mediate the adjustments." (*Ibid.*, p. 269.) He rejected teleological explanations of why men behave, argued that mentalistic and subjectivistic entities such as feelings and motives impede rather than advance useful inquiry, and concentrated on observed group behavior: "the raw material for the study of government. . .is first, last, and always activity, action, 'something doing' . . . . It must be taken as it comes in many men together. It is a 'relation' between men." (*Ibid.*, pp. 175-176.) For a sympathetic appraisal of Bentley's influence on a variety of behavioral fields, see *Life, Language, Law: Essays in Honor of*

Arthur F. Bentley, edited by Richard W. Taylor (Yellow Springs, Antioch Press, 1957).

David B. Truman, following Bentley's general procedures, has studied "the role of political groups in the governing process." (*The Governmental Process*, New York, Knopf, 1951.) Although Truman, like Bentley, focuses on interest groups, he is far more concerned than was Bentley with unorganized interests or "rules of the game." Such unorganized interests are attitudes or systems of belief rather than groups of people. They become part of our habit patterns as a result of early family and school experiences and exert a restraining influence on organized interest groups. Examples are the notions of justice, fairness, majority rule, etc. Bertram Gross attempted to describe on the basis of observed data the way in which bargaining and log-rolling elements function in the legislative process. He gave more attention to the role of individuals (their personalities, strategies, etc.) than did Bentley. (*The Legislative Struggle*, New York, McGraw-Hill, 1953.)

For good accounts of the type of behavioralism that developed in the 1950's and 1960's, see *Political Behavior*, edited by Heinz Eulau, Samuel J. Eldersveld, and Morris Janowitz (Glencoe, Free Press, 1956); *Legislative Behavior*, edited by John C. Wahlke and Heinz Eulau (Glencoe, Free Press, 1959); and *Essays on the Behavioral Study of Politics*, edited by Austin Ranney (Urbana, University of Illinois Press, 1962).

Such behavioralists focus not on the form and legal powers of government but on the politically oriented behavior of individuals and groups. They warn against premature system building and hold that the estrangement of theory and empirical research, even on lower levels of investigation, is a crippling flaw in much political inquiry. Recent behavioralist procedures of inquiry do emphasize the formulation of conjectures or hypotheses: "Its empiricism is, therefore, quite unlike the 'brute facts' approach of an earlier descriptive empiricism. It is self-consciously theory-oriented." (Eulau, Eldersveld, and Janowitz, *op. cit.*, p. 3.) At the same time, the behavioralists wish to be "quantitative wherever possible." In general, the behavioral trend:

> ". . .tries to develop rigorous research design and to apply precise methods of analysis to political behavior problems. It is concerned with the formulation and derivation of testable hypotheses, operational definitions, problems of experimental or *post-facto* design, reliability of instruments and criteria of validation, and other features of scientific procedures." (*Ibid.*, p. 4.)

Political science inquiry is viewed as continuous with other behavioral inquiry:

> "Although the study of political behavior is concerned with the actions of men and groups of men in politics, there are basic similarities between the actions of men and groups of men in other social institutions and situations. Consequently, many of the techniques and concepts developed, particularly by psychology, social psychology, and sociology, for the study of human behavior in general are applicable to the study of human behavior in politics." (Samuel J. Eldersveld *et. al.*, "Research in Political Behavior," *American Political Science Review*, Vol. 46, 1952.)

Beginning as a small protest movement, recent behavioralism rapidly became a major influence within political science, and is now sometimes criticized as being an "establishment" point of view. The prestige of presumed scientific procedures may be related to the recent emphases on the formal, logical side of scientific inquiry, as found, for example, in *The Methodology of Comparative Research*, edited by Robert T. Holt and John E. Turner (New York, Free Press, 1970). Such writers put much more emphasis on the logical empiricist notions about inquiry as developed by Rudolf Carnap, Carl Hempel, and others than on the Dewey-Bentley transactional procedures, which are opposed to formalism.

4. RESULTS ACHIEVED

What the problems and results of political science inquiry are is frequently a matter of dispute. As Heinz Eulau notes, there are not only many differing views as to what the field is, but disagreement as to the criteria by which conclusions are to be tested:

> "The history of political science as an independent field of inquiry can be written as a history of successive emancipations from earlier limitations and false starts. Yet, these successive emancipations have been additive rather than cumulative; the old survives with the new, and the old acquires new defenders as the new relies on old apostles. It is impossible to say, therefore, that anything has been disproven as long as conventional tests of proof—the requisites of scientific status in any field of knowledge—are not commonly accepted by political scientists, or, in fact, are rejected by some as altogether irrelevant in political inquiry." ("Political Science," in Bert F. Hoselitz, ed., *A Reader's Guide to the Social Sciences*, rev. ed., New York, Free Press, 1970, p. 135.)

In what follows, the major subfields of political science are reviewed to indicate the type of work being done and the type of conclusions reached.

Political Theory

As noted earlier, "theory" is used in several ways in the political science literature. Those adopting scientific procedures often use "theory" to refer to scientific conjectures, and emphasize testing, the emergence of theory out of observational data, etc. Some political scientists engage in the analysis of the work of famous writers in the history of political theory. In that context, "theory" usually refers to discussions of issues associated with traditional and speculative political philosophy, such as the ends of government, the nature of sovereignty, the types of political authority, and the analysis of ideologies.

Other inquirers have attempted to develop a general theoretical system or framework that is linked deductively to observable events. George E.G. Catlin, for example, earlier used a postulational method something like that of Spinoza and Hobbes in his *The Science and Method of Politics* (New York, Knopf, 1927). That procedure was elaborated in his *Systematic Politics* (Toronto, University of Toronto Press, 1962), which encompassed both political science and political theory (viewed as a study of values). The volume is prefaced by fifty-three propositions progressing through the scientific aspects to the value aspects. Although there was considerable emphasis on logic, Catlin also attempted to relate the logical implications to observational data. As noted earlier, other political scientists also have emphasized deductive models, particularly those from economics and game theory.

In recent years, the rise of "new left" criticisms of the relevance of most work in political science has been associated with a strong interest in the development of "normative theory" and a questioning of the basis for obedience and loyalty. Much interest has been shown in anarchist views.

There also has been an attempt to combine work on relevant and normative questions with logical analysis, as in Robert Dahl's *Preface to Democratic Theory* (Chicago, University of Chicago Press, 1956); Hannah Pitkin's *The Concept of Representation* (Berkeley, University of California, 1967); and David Braybrooke's *Three Tests for Democracy* (New York, Random House, 1968). Recently some political scientists have argued that normative theory and scientific inquiry are compatible. Ithiel de Sola Pool, for example, holds that "normative political theory and empirical political science are not contestants" and should be viewed "as a team in tandem when brought to bear on public policy." (Ithiel de Sola Pool, ed., *Contemporary Political Science: Toward Empirical Theory,* New York, McGraw-Hill, 1967, p. 230.)

Public Administration

In the field of public administration there are at least three major areas: (1) organization of governmental structures; (2) behavioral, sociological, and psychological aspects of administration; and (3) relationships of politics, administration, and policy formation.

Among the problems investigated are the objectives, goals, and motives of personnel, the specialization and division of labor, authority and control, communication, organizational decision-making, etc. The administrative importance of small groups and informal face-to-face associations is studied. Psychological and aptitude tests have been widely used for assessing qualifications. Administrative studies are often client-oriented and directed toward specific problems in a particular context. Frank G. Goodnow, Frederick W. Taylor, Leonard D. White, Luther Gulick, and John M. Baus, among others, made significant early contributions. For a review of those developments, see Dwight Waldo, *The Administrative State* (New York, Ronald, 1948).

The most influential recent figure probably has been Herbert A. Simon, who was one of the original popularizers of the language of decision-making and who tried to provide a scientific basis for "efficient" and "rational" decisions. Simon's account of administration regarded organizational leaders as continually striving to adjust to the political context and to achieve an equilibrium. His work draws from many other behavioral science areas. (See Simon's *Administrative Behavior,* 2nd ed., New York, Macmillan, 1957, and James G. March and Herbert Simon's *Organizations,* New York, Wiley, 1958, which summarizes many of the types of inquiry in this field.)

In recent years there has been strong interest in comparative problems of public administration, especially in developing areas (see Fred Riggs, *Administration in Developing Countries,* Boston, Houghton Mifflin, 1964), and a renewed interest in budgeting processes (see Aaron Wildavsky, *Politics of the Budgetary Process,* Boston, Little, Brown, 1964).

Voting Behavior

Many voting studies use panel surveys based on repeated interviews and rely upon sociological and psychological techniques and findings. Voting often is related to relatively localized conditions, and the generalizations reached often have only restricted application. Predictions derived from public opinion polling are apt to be highly limited.

Among the important studies of voting behavior are *Voting,* by Bernard B. Berelson, Paul F. Lazarsfeld, and William N. McPhee (Chicago, University of Chicago Press, 1954), and *The Voter Decides,* by Angus Campbell, Gerald Gurin, and Warren E. Milier (Evanston, Row, Peterson, 1954). *The American Voter,* by Angus Campbell, Philip E. Converse, and Donald E. Stokes (New York, Wiley, 1960), tries to account for voting decisions and to discover the connection between public opinion and antecedent conditions. The earlier work of Campbell and others in the Survey Research Center at the University of Michigan is elaborated in *Elections and the Political Order* (New York, Wiley, 1966), which also contains comparative materials. Data banks have been developed on both the national and international levels. (See Stein Rokkan, ed., *Comparative Research Across Nations and Cultures,* New York, Humanities Press, 1968.)

V.O. Key, Jr. attempted a broad synthesis of voting behavior findings in his *Public Opinion and American Democracy* (New York, Knopf, 1961). His posthumous book, *The Responsible Electorate* (with Milton Cummings, Cambridge, Harvard University Press, 1966) argues that "the voter is no fool"; Key is critical of the "irrational voter" notions held by some inquirers.

Legislative Behavior

"By legislative behavior is meant not only conduct in the performance of the legislative role, but also those attitudes and perceptions which relate to the process and substance of legislation." (Wahlke and Eulau, *op. cit.,* p. 5.) Numerous studies have been made of the psychological and sociological bases of legislative behavior and of the historical and institutional backgrounds. Considerable attention has been given to the origins, backgrounds, and attitudes of legislators and their occupational, ethnic, income, class, sex, and age distributions. Also studied are legislative practices, the influence of parties and pressure group, representation systems, etc.

On the national level, Richard Fenno studied the relation of the committee system to the larger legislative process in *Power of the Purse* (Boston, Little, Brown, 1966). For other developments, see Robert L. Peabody and Nelson W. Polsby's *New Perspectives on the House of Representatives* (2nd ed., Chicago, Rand McNally, 1969).

The Legislative System, by John C. Wahlke *et. al.* (New York, Wiley, 1962) explored state legislative processes and the self-images of legislators. *Politics in the American States,* by Herbert Jacob and Kenneth N. Vines (Boston, Little, Brown, 1965) compares many aspects of state governments. Studies of the links of public policy in the states to population, wealth, etc., are contained in Austin Ranney, ed., *Political Science and Public Policy* (Chicago, Markham, 1968.)

Judicial Behavior

Jack W. Peltason's study, *Federal Courts in the Political Process* (Garden City, Doubleday, 1955), was Bentleyan in orientation and related judicial decisions to the judges' interest affiliations. Clement Vose, Joel Grossman, Samuel Krislov, and many others have inquired into group influences on judicial selection and judicial decisions. Others, such as Glendon Schubert, have attempted to apply psychometric models to judicial attitudes and decisions. There have been studies of state, federal, and Supreme Court judicial behavior. For some representative recent work, see Stuart Nagel, *Legal Process from a Behavioral Standpoint* (Homewood, Dorsey, 1969), Kenneth Dolbeare, *Trial Courts in Urban Politics* (New York, Wiley, 1967), Glendon Schubert and David Danelski, eds., *Comparative Judicial Behavior* (New York, Oxford University Press, 1969), and Walter F. Murphy and Joseph Tanenhaus, *The Study of Public Law* (New York, Random House, 1972).

Parties, Pressure Groups, Public Opinion

The various groups and processes linking the formal machinery of government to the individual citizen have been investigated by many inquirers.

In studies of political parties and pressure groups, topics similar to those mentioned under voting, legislative, and judicial behavior are investigated. For example, in *Politics, Parties and Pressure Groups* (4th ed., New York, Crowell, 1958), V.O. Key, Jr. deals with agricultural, labor, business, and other interest groups; the nature and functioning of the party system; party organization, machinery, and leadership; campaigns, elections, and the electorate; the political aspects of administration; etc. Other political scientists have used organizational theory in studying political parties, and considerable work has been done on comparative party systems (for example, Leon Epstein, *Political Parties in Western Democracies*, New York, Praeger, 1967).

As noted, considerable work on interest groups was stimulated by Bentley's work (e.g., Truman's *The Governmental Process*). The pluralistic approach of Truman, Dahl, and others has been criticized in works such as Theodore Lowi's *The End of Liberalism* (New York, Norton, 1969) and Grant McConnell's *Private Power and American Democracy* (New York, Knopf, 1966). Mancur Olson reexamined the relation of individual and group interests in *The Logic of Collective Action* (Cambridge, Harvard University Press, 1965), in a way opposed to many Bentleyan notions.

Public opinion research has become a sizable field and involves interdisciplinary inquiry into individual and group attitudes, preferences, etc. (See, for example, Lloyd A. Free and Hadley Cantril, *The Political Beliefs of Americans: A Study of Public Opinion*, New Brunswick, Rutgers University Press, 1967, and John P. Robinson, Jerrold G. Rusk, and Kendra B. Head, *Measures of Political Attitudes*, Institute for Social Research, University of Michigan, 1968.)

Psychology, Sociology, and Anthropology of Politics

Harold D. Lasswell's introduction of psychological and personality theories into political science materials had considerable influence. In *Psychopathology and Politics* (Chicago, University of Chicago Press, 1930), Lasswell gave a psychoanalytic interpretation of "political man," who was viewed as fundamentally abnormal. His *Power and Personality* (New York, Norton, 1948) discussed "politicized" human relations. Numerous other studies focusing on personality and leadership have been made. Lewis J. Edinger summarizes many such studies in his biography, *Kurt Schumacher* (Stanford, Stanford University Press, 1965).

Many psychological investigations of propaganda and persuasion have also been made. Content analysis studies, aided by computer technology, have been widely used.

Political sociology focuses on groups, organizations, and institutions as influences on political life. See, for example, Seymour M. Lipset's *Political Man: The Social Bases of Politics* (Garden City, Doubleday, 1960). Considerable emphasis is found on participation in politics; see Robert E. Lane, *Political Life: Why People Get Involved in Politics* (Glencoe, Free Press, 1959) and Joseph Schlesinger, *Ambition and Politics* (Chicago, Rand McNally, 1966).

There have also been studies relating political science and anthropology, including work on the political structures of different cultures and on the influence of cultural patterns on political processes. See, for example, Georges Balandier, *Political Anthropology* (New York, Pantheon, 1971), and Claire Holt, ed., *Culture and Politics in Indonesia* (Ithaca, Cornell University Press, 1972).

Comparative Studies

We noted earlier the work sponsored by the Social Science Research Council's Committee on Comparative Politics. Numerous symposia on topics such as development, communication, and parties have resulted. Almond's advocacy of anthropological and sociological techniques and perspectives in the comparison of political systems, and the necessity to go beyond the regional, democratic or dictatorial, two-party or multi-party type of classification, has been influential. (G.A. Almond, "Comparative Political Systems," *Journal of Politics*, Vol. 18, 1956.)

Work has also been done on relating types of governance to economic development by Barrington Moore (*Social Origins of Dictatorship and Democracy*, Boston, Beacon Press, 1966). See also Samuel Huntington's *Political Order in Changing Societies* (New Haven, Yale University Press, 1968), and Zbigniew Brzezinski's *Between Two Ages* (New York, Viking, 1970).

Urban Government and Community Studies

The contemporary concern with urban problems has revived interest in this area. Robert Wood's *1400 Governments* (Cambridge, Harvard University Press, 1961) is an example of a systematic examination of relevant problems, especially in the New York metropolitan area. The question of the degree to which power is concentrated or dispersed has also attracted much attention. Robert Dahl, in *Who Governs?* (New Haven, Yale University Press, 1961) found a pluralistic dispersal of power, while others argued there was a control by elites (for example, see Robert Agger, Daniel Goldrich, and Bert Swanson, *The Rulers and the Ruled*, New York, Wiley, 1964).

American Government

This enormous field of investigation encompasses all American governmental levels (federal, state, local) and branches (executive, judicial, legislative). In *American State Politics* (New York, Knopf, 1956), V.O. Key, Jr.

used behavioral and analytic techniques to study the role of state governments, state politics, party organization, political leadership, primaries, etc. The Presidency has been a favorite topic. See Edward S. Corwin, *The President: Office and Powers* (4th ed., rev., New York, New York University Press, 1957) and Richard Neustadt, *Presidential Power* (New York, Wiley, 1960).

International Relations

International tensions since World War I have helped to make this an extensive area of inquiry. Some writers approached their materials from a particular ideological viewpoint, such as a "one world" commitment. Others have been concerned with the "realities" of politics. Hans Morgenthau, George F. Kennan, and others have been concerned with the conduct of foreign policy, the techniques by which it is executed, and ways to adjust and accommodate power conflicts among nations.

Recently efforts have been made to apply behavioral procedures of inquiry to international tensions and relations. J. David Singer, for example, discussed the uses and limits of sociology, anthropology, and psychology. ("The Relevance of the Behavioral Sciences to the Study of International Relations," *Behavioral Science*, Vol. 6, 1961.) Others have been interested in decision-making and game theories. See, for example, Richard C. Snyder, H.W. Bruck, and Burton Sapin, eds., *Foreign Policy Decision-Making* (New York, Free Press, 1962). Karl Deutsch has inquired into communication and other phenomena as indices of interaction. (*Nationalism and Social Communication*, Cambridge, M.I.T. Press, 1953, and *The Nerves of Government*, New York, Free Press, 1963.) Quantitative research has been done on a cross-national scale. See A.S. Banks and R.B. Textor, *A Cross Polity Survey* (Cambridge, M.I.T. Press, 1963); Ted R. Gurr, *Why Men Rebel* (Princeton, Princeton University Press, 1970); and John V. Gillespie and Betty A. Nesvold, eds., *Macro-Quantitative Analysis* (Beverly Hills, Sage, 1971).

5. CONTEMPORARY CONTROVERSY

In a field such as political science many controversies arise; our emphasis will be on general methodological disputes.

The Extent to Which Political Science Can Be Scientific

Some believe that political science inquiry can be entirely scientific, others that only certain aspects of the field are amenable to scientific inquiry, and still others that by far the most important aspects of the field are best investigated in some nonscientific fashion.

The controversy is exacerbated because of different applications of the name "science." Certain writers maintaining that political science is or has been scientific mean primarily that work in the field has been carefully done, that great efforts have been made to achieve internal logical consistency, and that the speculative system developed is relevant to the problems that it was intended to cover. Others emphasize deductive model building and consider that scientific. Yet others emphasize the type of modern inquiry discussed in Chapter I of this book.

More than in many other fields, there is a related disagreement about the role of the history of political thought. Sheldon Wolin argues:

> "In studying the writings of Plato, Locke, or Marx, we are in reality familiarizing ourselves with a fairly stable vocabulary and a set of categories that help orient us towards a particular world, the world of political phenomena. But more than this, since the history of political philosophy is an intellectual development wherein successive thinkers have added new dimensions to the analysis and understanding of politics, an inquiry into that development is not so much a venture into antiquarianism as a form of political education." (*Politics and Vision*, Boston, Little, Brown, 1960, p. 27.)

Others, primarily the behavioralists, strongly criticize such historical emphases. They may make considerable use of historical data, and have no objections to using promising materials from political theory or elsewhere, but they reject the notion that historical insights can be accepted without scientific confirmation.

Theorizing and Observation of Data

Within the group of political scientists who attempt to adopt scientific procedures of inquiry, roughly the same range of opinions are found about the relation of theorizing to observation that are found in other behavioral fields.

Some political scientists advocate the formulation of general theories of broad potential application, analogous to the theory of relativity in physics. Such formalized political theories are partially based on the observation of data, but often also are partially derived from what are believed to be sound general assumptions concerning human behavior, and are intended to be testable in principle. Emphasis on such formal theories (whether in the form of deductive theories or of mathematical or statistical models) has been challenged by critics who are not impressed either by the possibility of testing the conjectures involved or by the predictions that are deduced from them. Some of the critics emphasize what sociologists call "middle range theories," which are intended to be testable and yet not trivial. Others take the point of view suggested in Chapter I of this book, and believe that the continuous interweaving between developing conjectures, on the one hand, and observation, measurement of changes, etc., on the other hand, is the most useful procedure.

In view of such controversies, the remarks of a sociologist reviewing some recent methodological work by political scientists is of interest. Sanford Labovitz notes that some political scientists:

> "...write as if they have just discovered axiomatic theory, the philosophy of science, paradigms, models, logic, and mathematics (*not* including statistics). The authors overstress these aspects in their attempt to refocus political science along the scientific images of physics or biology. They emphasize, for the most part, only the formal or theoretical side of science and neglect...the research and data analysis dimensions." (*American Sociological Review*, Vol. 36, 1971, p. 329.)

Labovitz also suggests that "perhaps a fairly large number of political scientists have just moved into a stage that many sociologists have been in for 10 to 20 years or more." Of those who "emphasize formal axiomatic theory, model building, taxonomies, and paradigms," Labovitz says: "Rather than making the field of political science more scientific, they are likely to make it a sterile theorizing dead end. Theory, measurement, and data develop together; each supports the other." (*Ibid.*, p. 329, p. 332.)

Normative Political Science

Some traditionalists believe that political scientists can ascertain in some nonscientific way what the good society is; they view political science as basically a normative discipline.

Within the group advocating scientific methods, practically all agree that political values, in the sense of the preferences, objectives, etc., actually held by persons or groups, can be investigated scientifically. Many maintain that scientists *qua* scientists cannot determine what political values ought to be chosen; i.e., that scientific inquiry is ethically neutral. Such a view has been criticized by some who regard themselves as scientific, often in the context of advocating a "policy science" point of view. Policy science supporters sometimes emphasize the importance of a study of probable consequences for the selection of policy, which is compatible with the "ethical neutrality" point of view, but sometimes believe that normative political decisions themselves can be made within scientific inquiry. The entire issue is complicated by vague and inconsistent uses of "value" and "science.

In recent years, "value-free" behavioralism has been attacked by "new left" critics on the ground that ethical neutrality masks value commitments and that behavioralists in practice are irresponsibly or cynically serving the interests of dominant social groups. (See Charles McCoy and John Playford, eds., *Apolitical Politics*, New York, Crowell, 1968; and Philip Green and Sanford Levinson, eds., *Power and Community*, New York, Pantheon, 1970.)

Is Political Science a Separate Field?

Political inquiry generally has been closely connected with work done in other fields. Recent developments have led both some political scientists and other behavioral scientists to maintain that political science is not a separate area of inquiry and that the problems investigated by political inquirers fall logically into other behavioral fields such as sociology, psychology, and anthropology. The most important of such developments was the success of the behavioralists in using the techniques of other behavioral areas to inquire into political processes; another factor was the widening of the description of the field of political inquiry to include behavior that conventionally would not be regarded as primarily political. Other observers, especially those who deny the usefulness of scientific procedures, insist that political science is fundamentally an autonomous field, however much it may draw upon other areas. (For discussions about the relation of political science to other behavioral areas, see Seymour M. Lipset, ed., *Politics and the Social Sciences*, New York, Oxford University Press, 1969.)

Such controversies are related to a more general problem in behavioral inquiry about the division and specialization of labor. Both the investigative techniques and the descriptions of presumed subject matters cut across the conventionally differentiated behavioral fields; inquiry into the behavior of men-in-society cannot be compartmentalized into separate fields requiring differing methodologies. The division of research along the lines of separate disciplines often tends to impede inquiry.

Perhaps most contemporary scientifically inclined inquirers would agree with the thrust of Lipset's comments:

> "Many political scientists, particularly in recent years, have argued, sometimes with others in their own field, that it is impossible to study political processes except as special cases of more general sociological and psychological relationships. The increasing collaboration, as well as the acceptance of common concepts and methods, among those studying political behavior within the fields of political science, sociology, psychology, and anthropology (each of the latter three now having a recognized sub-discipline dealing with politics) is new evidence of the basic unity of the social sciences. The study of man in society cannot fruitfully be compartmentalized according to substantive concerns." (Lipset, 1960, *op. cit.*, p. 9.)

6. TERMINOLOGICAL PROBLEMS

Despite the many efforts to clarify terminology throughout the history of political science, problems abound. Often clarification meant only making the terminology consistent within some speculative system. The earlier emphasis on notions such as "state" and "sovereignty" yielded many conflicting descriptions of what was involved, and often the processes being named were reified. As an illustration of the disagreements, C.H. Titus found 145 separate "definitions" of "state." ("A Nomenclature in Political Science," *American Political Science Review*, Vol. 25, 1931.)

The recent emphasis on political behavior as the subject matter of inquiry has called attention to the importance of terminology referring to behavioral processes. Felix Oppenheim, for example, says that probably the most important task of political scientists is the development of a "satisfactory vocabulary for the description of human behavior." (Oppenheim, *op. cit.*, p. 5.)

As is the case in other behavioral fields, there are problems about the basic terminology for describing behavior. As discussed in the Chapter on Psychology and elsewhere, there are disagreements as to whether behavior should be viewed as overt only, and as "belonging to" the organism involved, or is more usefully viewed as including all organic-environmental adjustmental transactions. Oppenheim, for example, takes behavior as overt and as excluding processes such as preferring and believing, although others would use "behavior" to include what he excludes:

> "Behavior may be defined as any bodily movement of an organism, animal or human—including, of course, verbal behavior. Behavior, by definition overt, must be distinguished from mental processes—for example, believing, inferring, preferring, intending—which may or may not terminate in some physical doing." (*Ibid.*, p. 15.)

Given the important differences and confusions about basic nomenclature that are closely connected to disagreements about methods, the terminology used in the problems political scientists are most interested in is almost certain to be involved in controversy also.

Although many illustrations could be given of terminological disagreement within characteristic political science inquiry, only one will be mentioned here. As noted, many (but not all) political scientists emphasize "power" as basic to their subject matter. Accounts of what "power" most usefully names vary widely. Some are nonformal; Charles Eisenmann's statement is representative of such approaches. He says that political power is "the effective ability to regulate human conduct with the backing of sanctions, implying, where necessary, recourse to force." ("On the Matter of Methods of the Political Sciences," in *Contemporary Political Science*, Paris, UNESCO, 1950, p. 82.)

Others find such accounts somewhat vague and offer more formalized statements. Lasswell and Kaplan, for example, give a partially formalized account of power as the "participation in the making of decisions: G has power over H with regard to value K if G participates in the making of decisions affecting the K-policies of H." (Lasswell and Kaplan, *op. cit.*, p. 75.)

Others believe a more rigorous formulation is necessary so that precise rankings in power can be made. Robert Dahl, for example, developed a symbolic notation for his notion of power as a relation between people, and then ranked U.S. Senators according to their power over legislation. ("The Concept of Power," *Behavioral Science*, Vol. 2, 1957.) Numerous other formulations can be found, and doubtless additional ones will continue to be developed. Political scientists often have not focused on names that are useful in describing observable behavior, but on "definitions" that make for neat, consistent, and comprehensive systems.

7. COMMENT AND EVALUATION

As noted in other Chapters, a strong movement favoring a return to subjective methods has occurred in recent years in several fields in which purportedly scientific procedures of inquiry had been dominant, or at least influential, for some time. Although similar tendencies are found in political science, at least on the surface the commitment to scientific inquiry still is widespread and the "return to subjectivism" movement may be less influential than in some other fields.

If one looks more carefully, however, some of the allegedly scientific procedures of recent years have been primarily concerned with "theory development" far in advance of the available observational data, and there seems to be a strong tendency to "test" theory by the intellectual satisfaction or initial plausibility it may have. Sometimes, indeed, the gap between available data and a model is regarded as a virtue. For example, in a review of Tullock's *Toward a Mathematics of Politics*, Joseph L. Bernd says the following about logical model building:

> "A chief advantage of this approach, whether in behalf of pure science or policy formulation, is that the variables which are fed into the model may be derived from the entire range of logical possibilities and are not narrowly limited to the usable data which fall to hand in the world of sensory experience." (*Political Science Quarterly*, Vol. LXXXV, 1970, p. 126.)

Much the same might be said in behalf of traditional "theories" in political philosophy. At a given moment there may be insufficient reliable data to allow the development of a conjecture that can be thoroughly tested, but the remedy hardly seems to be an emphasis on "theorizing" *per se*. Before Galileo rolled balls down an inclined plane, inquirers did not have the data enabling them to formulate warranted assertions about falling bodies. The traditional Aristotelian doctrine had the "advantage" of allowing "the entire range of logical possibilities" to be fed into it, and was not restricted to "sensory experience," but also failed to lead to prediction and control. Galileo's achievement was to get appropriate data for the problem at hand and then to formulate a generalization that could be tested further.

Moreover, even when there is an emphasis on deriving predictions about the "real world" from a formal model, often what is taken as a confirmation of the model is not adequate. In view of the prominence of models in recent political science inquiry, looking at one example in some detail may be useful.

In the work by Riker and Zavoina cited earlier, subjects (primarily college students) were involved in a three person game in which coalitions were formed after negotiations between pairs of the subjects. The coalitions received $6.00, $5.00, and $4.00 if three types of coalition were formed; otherwise nothing. The subjects' behavior in the game very often conformed closely to the von Neumann-Morgenstern solution for the type of game being played. Overall, from 92% to 95% of the subjects "tried to maximize the probability of winning," and the majority of the others "were apparently trying to maximize where some alternative other than winning stood higher in their order of preference." (Riker and Zavoina, *op. cit.*, p. 56.)

We have, then, a situation in which observed behavior conforms to some notions about rationality and utility maximization; how does that behavior compare to the behavior of politicians? The authors admit that their study deals only with "surrogate politicians in a surrogate political setting." They discuss many of the ways in which the analogy between their game and politics fails, and they tried to make their game as "realistic" as possible by introducing "putative equivalents" for what would be found in political situations. They also say, however, "we know, of course, that these putative equivalents are pretty pale imitations of these forces in political life." (*Ibid.*, p. 59, p. 52.)

Given all those considerations, the authors still conclude that "it is safest to assume that politicians are calculating maximizers"; that although their evidence is indirect, the "message" from the evidence is "crystal clear" that in situations where choice is possible "utility maximization is the theory that fits political behavior best," and that until politicians can be induced to answer what they "would surely regard as silly" questions, "games in the laboratory are about the best we can do to study political behavior in exhaustive detail." (*Ibid.*, p. 60, p. 52.)

Many logical models are faced with a similar type of problem. If the behavior of interest cannot be inquired into directly, a model presumably analogous to that behavior in important ways may be constructed as a surrogate. But if the "target behavior" *cannot* be inquired into, how can the predictions from the model be confirmed? If it turns out that the "target behavior" can be studied directly, as would be required to confirm predictions, perhaps an elaborate model was not necessary or even useful in the first place.

The recent emphasis on theoretical models among political scientists has some similarity to the older nonscientific procedures of inquiry. The importance of many policy decisions for so many humans obviously makes it desirable to base policy on the best findings available. But fairly often the warranted assertions necessary for reliable practical use simply have not been developed as yet. Then looking for a "short cut" through insufficiently tested conjectures is tempting.

In general, in the last two decades there has been a marked shift among American political scientists away from a focus on legal forms and the structural aspects of institutions to a focus on behavioral processes. Such processes have been investigated by inquirers using transactional, interactional, and sometimes self-actional procedures. The scientific results to date have been limited, but the prospects seem promising, especially if political scientists do not succumb to the temptation (seemingly attractive to many) of focusing their efforts on the formulation of elaborate conjectures far removed from observation of facts and measurement of changes. The "behavioralists" apparently have abandoned many of the outmoded procedures of inquiry; if they focus inquiry on the relevant sign-behavior and avoid the pitfall of formulating pseudo-scientific conjectures, progress may become rapid.

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INTERNATIONAL POLITICAL SCIENCE ABSTRACTS
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PUBLIC OPINION QUARTERLY
REVIEW OF POLITICS
SOCIAL RESEARCH
STATE GOVERNMENT
URBAN AFFAIRS QUARTERLY
WESTERN POLITICAL QUARTERLY
WORLD POLITICS

VI.

ECONOMICS

1. WORKING DESCRIPTION OF THE FIELD

ECONOMISTS inquire into human behavior involved in obtaining and modifying things that can be used for food, clothing, shelter, and other purposes; behavior that includes the processing of things, including distribution and exchange, and the consumption of scarce commodities and services, most of which have alternative possible uses. Many aspects of economic behavior overlap with inquiries made by sociologists, psychologists, political scientists, anthropologists, historians, and other behavioral scientists.

2. OTHER DESCRIPTIONS OF THE FIELD

The field of economics has been described in many different ways. At times a broad view of the field is taken. For example, Alfred Marshall viewed economics as "a study of man's actions in the ordinary business of life." (*Principles of Economics*, Vol. I, New York, Macmillan, 1890, p. 1.) Ludwig von Mises said: "Economics, as a branch of the more general theory of human action, deals with all human action, i.e., with man's purposive aiming at the attainment of ends chosen, whatever these ends may be." (*Human Action: A Treatise on Economics*, New Haven, Yale University Press, 1949, p. 880.) Other writers have described the field more narrowly.

According to L. M. Fraser, there are two major groups of descriptions found in the literature: "The first connects the concept with wealth, or welfare, the second with scarcity." (*Economic Thought and Language*, London, Adam and Charles Black, 1947, p. 21.) The emphasis on *wealth* is characteristic of many early economic treatises, e.g., Adam Smith viewed economics as the "science of wealth," or as an "inquiry into the nature and causes of the wealth of nations." (*The Wealth of Nations*, 1776.) More recently, *scarcity* often has been the focus of attention; e.g., Lionel Robbins held that economics "is the science which studies human behavior as a relationship between ends and scarce means which have alternative uses." (*An Essay on the Nature and Significance of Economic Science*, rev. ed., London, Macmillan, 1935, p. 16.)

Sometimes the notion of *maximizing* is made central:

> "Economics is the study of the principles governing the allocation of scarce means among competing ends when the objective of the allocation is to maximize the attainment of the ends." (George J. Stigler, *The Theory of Price*, New York, Macmillan, 1946, p. 12.)

And Bert F. Hoselitz says:

> "One central feature of all economic problems, theoretical and applied, is that they all involve a process of maximization (or minimization) in the realm of social action. This may be expressed differently by saying that an economic problem exists if some social end is given and has to be achieved with a minimum of expenditure of means, or if a set of means is given and a maximum result is to be achieved. Thus, an economic problem arises whenever scarce resources must be allocated among alternative uses." ("Economics," in Bert F. Hoselitz, ed., *A Reader's Guide to the Social Sciences*, rev. ed., New York, Free Press, 1970, pp. 241-242.)

The *institutionalists* emphasized the influence of other social institutions on economic institutions and behavior:

> "If we define economics as the study of man's behavior in making a living, it would seem relatively easy to designate the relevant institutions. But man doesn't divide his life into separate compartments. His activity in politics, religion, and social life cannot be completely disassociated from his economic activity. He acts as an entire organism, and the influences of one sphere of activity do not completely disappear when he goes into other activities. Consequently, the realm of pure economic institutions cannot be isolated." (Donald W. McConnell *et. al.*, *Economic Behavior: An Institutional Approach*, rev. ed., Boston, Houghton Mifflin, 1939, pp. 902-903.)

Some authors attempt to describe the field so as to include not only market economies in which prices play a major role, but also relatively simple and unindustrialized systems and collectivist economies. Morris A. Copeland, for example, suggests that "economics should be defined as the study of economies," and further says:

> "An *economy* is a particular type of social structure, a type that is concerned with the problems of social organization arising out of. . .the *division of labor*. . . . [The] division of labor consists in the fact that the work to be done in the community or society is divided up so that different specialists do different parts of it. . . . An economy, then, is a social structure that coordinates the activities of the various specialists in a community or society. And by *community* or *society*. . .we mean a social group living in a particular geographical area—a group that is inclusive enough to be more or less self-sufficient, so that the most of those wants of its members that other people are expected to satisfy are satisfied through the labors of other members of the group rather than outsiders." (*Our Free Enterprise Economy*, New York, Macmillan, 1965, pp. 15-16.)

Other descriptions focus attention on behavior more characteristic of economic systems similar to our own. Nancy D. Ruggles, for example, says:

> "Since [Adam Smith], economists have sought to explain the behavior of the economic system as an interacting mechanism. Smith concerned himself with the interdependence of wages, profits, rents, prices, money, and capital accumulation. These are the same topics which primarily interest economists today. The long-range goal most economists recog-

> nize as central to the discipline is the development of an understanding of precisely how the economic system operates—the mechanisms by which resources are allocated, prices determined, income distributed, and economic growth takes place." ("The Goals and Achievements of Economics," in Nancy D. Ruggles, ed., *Economics*, The Behavioral and Social Sciences Survey sponsored by the National Academy of Sciences and Social Science Research Council, Englewood Cliffs, Prentice-Hall, 1970, p. 3.)

Paul Samuelson, in his widely-used textbook, says that contemporary economists "agree on a general definition something like the following":

> "Economics is the study of how men and society end up *choosing*, with or without the use of money, to employ *scarce* productive resources which could have alternative uses, to produce various commodities and distribute them for consumption, now or in the future, among various people and groups in society." (*Economics*, 8th ed., New York, McGraw-Hill, 1970, p. 4.)

3. METHODS AND TYPES OF INQUIRY

Various ways of classifying economists methodologically can be found. Classifications may be based on relatively specific techniques rather than general procedures of inquiry (e.g., when mathematical economists are differentiated from others); on the methods and techniques associated with a particular economist or group of economists (e.g., when the neoclassical methodology is differentiated from others); or on some combination of procedures and leading conclusions (e.g., Keynesian economics vs. other economics).

For present purposes, we will emphasize issues concerning the general procedures of inquiry, and consider specific techniques only in relation to those general procedures. From our perspective, procedures that often are regarded as distinctive methods (e.g., much of mathematical economics) are viewed instead as a continuation of certain traditional procedures. We also should note that a particular economist may combine (consistently or inconsistently) various aspects of the different procedures we discuss.

The Quest for Certainty

Although Adam Smith and other early classical economists frequently described economic events and the connections among them in a manner that would qualify as excellent work today, they also attempted to develop mutually consistent and presumed basic assumptions from which further conclusions about behavior could be logically derived. In the late nineteenth and early twentieth centuries, the neoclassical economists emphasized the logical implications of assumptions based on common sense, introspection, and presumed universal traits of human behavior. Often their confidence in the "certainty" of the assumptions and the deductive rigor of their arguments was so great that they believed their conclusions did not require observational confirmation in order to be accepted.

Many later economists, who often disagree strongly about their findings and policy recommendations, also emphasize logical deductions from plausible premises. J. M. Keynes, for example, said:

> "For if orthodox economics is at fault, the error is to be found not in the superstructure, which has been erected with great care for logical consistency, but in a lack of clearness and of generality in the premises." (*The General Theory of Employment, Interest and Money*, New York, Harcourt, Brace & World, 1949 ed., p. v.)

According to Leland B. Yeager:

> "Often we cannot check economic theorems in the complex, 'real,' or 'macro,' world because the effects of numerous influences are hopelessly intermingled. Fortunately, however, we can check economic theorems in another way—a way not available to natural scientists. We check the postulates directly. That is, we *start* the chains of deductive reasoning from dependable knowledge, as of the scarcity of productive resources in relation to practically unlimited human needs and wants, of the law of diminishing returns, and of human motives and wants and choices. . . .In forming theories, we can sort out the influences of various factors on economic affairs because we know from personal experience how people react." ("Measurement as Scientific Method in Economics," *American Journal of Economics and Sociology*, Vol. 16, 1957, pp. 342-343.)

Perhaps the strongest recent confidence in rational certainty has been expressed by Ludwig von Mises:

> "What assigns economics its peculiar and unique position. . .is the fact that its particular theorems are not open to any verification or falsification on the ground of experience. . . . The ultimate yardstick of an economic theorem's correctness or incorrectness is solely reason unaided by experience." (Von Mises, *op. cit.*, p. 858.)

Other writers, although arguing in favor of observational testing in economics, also believe that certain assumptions about behavior will be acceptable to nearly all "normal" humans. Robert H. Strotz, for example, in a discussion of some "axioms" assumed in the measurement of utilities, says that the "axioms have strong intuitive appeal. It would seem that every normal person would clearly accept them as precepts of behavior." He admits that possibly a human might behave in a way inconsistent with the axioms, but adds that "it would be a strange man indeed who would persist in violating these precepts once he understood clearly in what way he was violating them." ("Cardinal Utility," in *Papers and Proceedings*, *American Economic Review*, Vol. XLIII, No. 2, 1953, p. 391, p. 393.)

In its strongest form, the quest for certainty can be described as follows: At least some important "knowledge" about human economic behavior can be achieved by beginning with premises known through "reason" to be "true," and then rigorously deducing consequences from those premises that will be as certain as the premises were. In weaker versions, there may be no requirement that the initial premises are "absolutely true," and the logical consequences derived may be said to be testable in principle by observation, but there still is confidence that the deductive elaboration of apparently sound and mutually consistent premises can lead to useful conclusions, even though the conclusions cannot be tested adequately.

Introspection and Empathy

The quest for certainty just described often is accompanied by an insistence that economists, unlike natural scientists, can arrive at some important "knowledge" about their field through introspection and the empathic interpretation of other people's behavior. Friedrich A. Hayek, for example, viewed economics as a subjective discipline on the grounds that human behavior must be understood from "within," and that we must interpret other people's behavior in "the light of our own mind." (*The Counter-Revolution of Science*, Glencoe, Free Press, 1952, pp. 44-45, p. 77.)

Sometimes a Kantian view of "mind" is adopted in which all human minds are said to have the same logical structure. Ludwig von Mises says that "it is impossible for the human mind to conceive logical relations at variance with the logical structure of our mind." Therefore, he goes on, to understand human behavior "there is but one scheme of interpretation and analysis available, namely, that provided by the cognition and analysis of our own purposeful behavior." (Von Mises, *op. cit.*, p. 25, p. 26.) And Yeager says:

> "It is understandable that to people trained in the natural sciences, the method of economic theory may smack of Kantianism and its synthetic *a priori*. . . . Anthropomorphism, rightly scorned in the natural sciences as pre-scientific metaphysics, is justified in economics because economics is about human action." (Yeager, *op. cit.*, p. 344.)

At other times, the emphasis is not on assumed *a priori* structures or on the "mind" *per se*, but on gaining presumed "knowledge" through imaginatively placing yourself in another's position. According to Samuelson:

> "In a sense, precisely because we are ourselves men, we have an advantage over the natural scientist. He cannot usefully say, 'Suppose I were an H_2O molecule; what might I do in such a situation?' The social scientist often, knowingly or unknowingly, employs such introspective acts of *empathy*." (Samuelson, *op. cit.*, p. 9.)

Model Building

a. Mathematical models. Although mathematical models of some sort have been used by economists for a long time, in recent years a substantial portion of the work in economic "theory" has consisted in developing such models. Wassily Leontief says:

> "[In 1953] high school calculus was viewed by most economists as advanced mathematics and matrix algebra was still relegated to the pages of the journal called *Econometrica.* Now, less than fifteen years later, mathematics has without doubt been recognized as the *lingua franca* of economic theory and most of the current work in the field of economic theory is deviated to proofs of formal theorems derivable from more or less arbitrarily chosen sets of axiomatic assumptions and, what is essentially the same, a large-scale production of new mathematical models." (*Essays in Economics*, New York, Oxford University Press, 1966, p. vii.)

Often the general procedure utilized is that of the older economists, in which the emphasis is on deduction, not on observational tests or measurements of change. According to Richard C. Bernhard:

> "In the social sciences, many models are mathematical formulations using type equations without specific numerical content. The most eminent of these is the Walrasian pure theory of economics, a logical structure of marvelous intricacy and great beauty, but by itself not based on measurements nor leading to experimental verification of deductions from its postulates." ("Mathematics, Models, and Language in the Social Sciences," *Symposia Studies Series No. 3*, National Institute of Social and Behavioral Science, George Washington University, 1960, p. 2.)

Sometimes the language used ("tested against reality," "observation of behavior") suggests that a model is being tested by something external to it, when actually the tests are primarily internal. E.F. Beach, for example, says: "Mathematical models are theoretical constructions which are *tested against reality* mainly on grounds of consistency and reasonableness." (*Economic Models: An Exposition*, New York, Wiley, 1957, p. 3. Italics added.) And Fritz Machlup, in a criticism of an article by Samuelson, says:

> ". . .Samuelson. . .characterized the problem as 'a purely logical one'. . . . But he sometimes uses language of empirical operations, for example, when he speaks of 'observing the behaviour of a representative firm.' It should be clear, however, that what he 'observes' is merely the logical consequence of a set of assumptions; that the 'behaviour' is purely fictitious; and that his representative firm is only an ideal type, a theoretical construct." ("Professor Samuelson on Theory and Realism," *American Economic Review*, Vol. LIV, 1964, pp. 734-735.)

b. Game theory. The founders of game theory, John von Neumann and Oskar Morgenstern, were critical of many of the older mathematical models: "frequently one is offered not proofs but mere assertions which are really no better than the same assertions given in literary form." Beginning with problems in games of strategy such as chess and poker, they attempted to develop models for "rational" economic behavior in which "mathematical treatment can be brought beyond the mere formalism to the point where it yields complete numerical results." A major objective was "to establish satisfactorily. . .that the typical problems of economic behavior *become strictly identical* with the mathematical notions of suitable games of strategy." (*Theory of Games and Economic Behavior*, 3rd ed., Princeton, Princeton University Press, 1953, p. 5, p. 32, pp. 1-2. Italics added.)

Solutions to a great many games have been developed. To illustrate briefly, in two-person "constant-sum" games (in which the algebraic sum of the gains and losses is constant), the solution involves each player's adopting the strategy in which minimal gain is at least as great as that provided by any other strategy.

Among the objections that have been made to game theory as descriptive of behavior is that assumptions necessary to the solutions offered are inconsistent with the actual situation (e.g., that the players have "complete knowledge" of the preference patterns of the other players), and that the analogy between the games and typical economic behavior fails (e.g., in many games whatever one person gains must be lost by others, but in many economic transactions there is a net gain for all participants). In recent years, defenders of game theory often have argued that the solutions are prescriptive, not descriptive; the solutions show how a rational person should behave in certain circumstances, not how humans do behave.

Game theory is discussed in more detail in Chapter X. For present purposes, we conclude by noting that whether game theory is viewed as descriptive or prescriptive, the main procedure of inquiry used in developing solutions is the deriving of consequences from assumptions in the hope that the consequences somehow will be useful for describing actual human behavior.

c. *Simulation models and computers.* Computers and other technological aids are increasingly used by economists. The following quotation from Robert M. Solow illustrates how far in advance of observation some economists are willing to proceed, and the relative importance they give to "theory":

> ". . .as economic theory is forced into models too complicated for mathematical solution, even the pure theorist will have to turn to computer experiments to discover the implications of his assumptions, and to compare them with *the crude facts*. . . . The need for computation services on the part of microeconomic theorists can arise in another way. When one formulates a theoretical description of the behavior of a single firm or household, there is likely to be an enormous variety of behavioral assumptions that can be made with some degree of plausibility. *The choice among them cannot wait for empirical research, which is likely to be difficult anyway.* It is often the case, however, that the end product of the theory is insensitive to certain of the behavioral assumptions. It is important to find out which of the assumptions fall into that category, and sensitivity analysis through computer simulation is a logical way to do so." ("Microeconomic Theory," in Ruggles, *op. cit.*, pp. 42-43. Italics added.)

In an effort to make elaborate conjectures more "realistic" and "scientific," considerable attention has been given to the development of computer simulation models of a firm, an industry, or a nation. (Thomas H. Naylor, *Computer Simulation Experiments with Models of Economic Systems*, New York, Wiley, 1971.) Much effort has gone into aggregate models of the United States economy, such as the Social Science Research Council-Brookings Institute model, the construction of which involved the cooperative efforts of many well-known economists. (For a brief description of some recent large-scale models, see Carl F. Christ, "Econometrics and Model-Building, 1967-1972," *The Annals of the American Academy of Political and Social Science*, Vol. 403, 1972.) The hope is that the models can be made useful enough so that important policy recommendations can be soundly based on the simulations. One important technique used in evaluating and improving a model is to simulate past periods of economic events and see how close the resulting predictions are to what actually happened. At times the predictions are confirmed, but at times great discrepancies are found. In his report on the Brookings model, Robert Gordon says of the simulation for the 1953-1962 period:

> "The results are very discouraging. While the model is able to generate the 1953-54 recession, it completely misses both the 1957-58 and 1960-61 recessions. The predicted change in real GNP between 1957:3 and 1958:2 is an increase of $6 billion, compared to an actual decline of $16 billion." ("The Brookings Model in Action," *Journal of Political Economy*, Vol. 78, 1970, p. 521.)

Apparently many recent economists agree that although in principle the consequences of deductions made from premises should be tested observationally, and that economic "knowledge" cannot be based on deduction and ratiocination alone, considerable emphasis may still be placed on the deductive elaboration of conjectures. Samuelson, for example, after criticizing the "exaggerated claims" made for the "power of deduction and a priori reasoning" by many earlier economists through von Mises, says that "we have left that behind us." He then discusses critically some of his own earlier (1948) work as follows:

> "From certain empirical hypotheses taken as postulates, by cogent logic I *deduced* as theorems certain other empirical properties. . . . When one looks at the complicated real world, one finds it obvious that the hypotheses of the syllogism are far from valid, and, also, the consequences are far from valid. This is indeed a matter for regret and full disclosure of inaccuracy should be made. Nevertheless. . .a strong polar case like this one can often shed useful light on factual reality. . . . Scientists constantly utilize parables, paradigms, strong polar models to help understand more complicated reality. The degree to which these do more good than harm is always an open question, more like an art than a science. . . . Nature [sometimes] seems to show an inexplicable simplicity. . . . As a result, the working scientist learns as a matter of routine experience that he should have faith that the more beautiful and more simple of two equally (inaccurate) theories will end up being a more accurate describer of wider experience." ("Theory and Realism: A Reply," *American Economic Review*, Vol. LIV, 1964, p. 736, p. 737, p. 739.)

In passing, we note our difficulty in understanding Samuelson's assertion that of two *equally inaccurate* conjectures, one may be a *more accurate* describer of behavior, unless "accurate" designates two different things in the same sentence. (More will be said on semantic problems in Section 6.) In any event, some economists who reject "rationalism" continue to apply "rationalistic" methods.

Attempts to Use Scientific Procedures in Economics

Unfortunately, "science" is a label that has been applied in such diverse ways that communication often is impeded. In the recent literature of the methodology of economics, scientific inquiry frequently is linked to observational testing, although major importance may still be given to "theory" development. A typical statement is from Bert F. Hoselitz:

> "Economists today tend to proceed in their

> research by a common method, which is in its chief aspects identical with the procedures of any empirical science, i.e., the testing of theories by relating them to empirically observable data." (Hoselitz, *op. cit.*, p. 264.)

At times the tentative character of all scientific conclusions is stressed. Andreas G. Papandreou, for example, says:

> "...hypotheses which occur in the theory as theorems must be capable (in principle) of being refuted by reference to empirical evidence. If the predictions incorporated in the hypotheses are not falsified by the empirical evidence, they may be adopted by the theorist—but in a tentative manner—for they are always capable of being refuted, by new empirical evidence." (*Economics as a Science*, Chicago, Lippincott, 1958, p. 7.)

Some writers emphasize what we call the interweaving of conjectures and observation. Leontief says:

> "...advance can be achieved only through an iterative process in which improved theoretical formulation raises new empirical questions and the answers to those questions, in their turn, lead to new theoretical insights." ("Theoretical Assumptions and Nonobserved Facts," *American Economic Review*, Vol. LXI, 1971, p. 5.)

Other economists have adopted views of scientific inquiry in which the importance of observation is minimized. For present purposes, we have selected for commentary some of the views in which observational testing is emphasized, although (as we shall see) there are important differences of opinion about what constitutes adequate testing.

Institutionalism and related developments. Certain American economists, who often are called institutionalists, severely criticized many traditional economic procedures and conjectures as pre-Darwinian. Although often disagreeing with each other, and sometimes engaging in the "quest for certainty," the early institutionalists attempted to bring together observation and conjecturing, argued for the importance of applying the findings of other behavioral sciences to economics, and emphasized the role of institutions (habitual and organized modes of conduct) in economic behavior.

That point of view was more fully developed by Wesley C. Mitchell, who urged a fundamental reconstruction of economics as a science:

> "If my forecast is valid, our whole apparatus of reasoning on the basis of utilities and disutilities, or motives, or choices, in the individual economy, will drop out of sight in the work of the quantitative analysts, going the way of the static state. The 'psychological' element in the work of these men will consist mainly of objective analysis of the economic behavior of groups. Motives will not be disregarded, but they will be treated as problems requiring study, instead of being taken for granted as constituting explanations." ("Quantitative Analysis in Economic Theory," *American Economic Review*, Vol. XV, 1925, p. 5.)

Although Mitchell used a different terminology than we do, his procedures of inquiry—unlike those of some of the more recent quantitative economists—agree with our suggestions in Chapter I concerning the interweaving of conjectures and observation. The National Bureau of Economic Research, which Mitchell founded and directed for many years, was concerned with the collection of data and the measurements of changes that would facilitate the development of conjectures closely linked to careful observations. Pure speculation, or only casually checked speculation, was rejected. For example, in a report relating the work of the Bureau to Keynesian notions, Arthur F. Burns said:

> "Fanciful ideas about business cycles are widely entertained both by men of affairs and by academic economists. That is inevitable as long as the problem is attacked on a speculative level, or if statistics serve merely as a casual check on speculation.... there is no reliable short cut to tested knowledge." (*The Frontiers of Economic Knowledge*, Princeton, Princeton University Press, 1954, p. 19.)

More recently, the National Bureau appears to have departed from the line of advance exemplified by Mitchell and has begun to support elaborate conjectures developed far in advance of observation. (See Rollo Handy and E.C. Harwood, *Useful Procedures of Inquiry*, Great Barrington, Behavioral Research Council, 1973, pp. 204-205.) The procedures of inquiry used by Mitchell have been continued and further developed by the American Institute for Economic Research:

> "The methods of conducting inquiries applied by the Keynesians and to a substantial extent by the classical economists were the older, now obsolete methods. Briefly, those methods included Aristotelian logic, introspection, what may be called secular revelation (a process at which Lord Keynes was especially adept), and the quest for certainty so long persisted in also by philosophers. Such methods give great weight to the internal logical consistency and general plausibility of an hypothesis but accord little weight to the desirability of testing its logical implications against measurements of economic changes before offering the hypothesis as a warranted assertion applicable to the problems of men." (E.C. Harwood, *Reconstruction of Economics*, 3rd ed., Great Barrington, American Institute for Economic Research, 1970, p. 12.)

Econometricians. The work of the econometricians is sometimes viewed as the mathematical-statistical link between the development of conjectures and a thorough test of those conjectures. According to Nancy Ruggles:

> "Econometrics represents the application of mathematical and statistical methods to the testing of hypotheses and the analysis of economic data. It has encouraged the mathematical formulation of economic theories so that hypotheses can be presented in a more rigorous form, capable of being tested.... Econometrics has thus given the economist a tool that bridges theory and empirical observation, making possible the testing of hypotheses concerning economic behavior and the operation of the economic system." (Ruggles, *op. cit.*, p. 6.)

Some observers, however, have been critical of the testing. Harry G. Johnson, for example, says:

> "Another [revolution in economics] was the empirical or econometric revolution, with its insistence initially on the measurement of economic relationships and, subsequently and more ambitiously, on the testing of economic hypotheses—though the 'testing of hypotheses' is frequently a euphemism for obtaining plausible numbers to provide ceremonial adequacy for a theory chosen and defended on *a priori* grounds." ("The Keynesian Revolution and the Monetarist Counter-Revolution," in *Papers and Proceedings, American Economic Review*, Vol. LXI, No. 2, 1971, p. 2.)

Tjalling C. Koopmans described the task of econometrics as follows:

> "(1) to formulate all relevant hypotheses to which the available data may conceivably make an answer possible; (2) to extract from those data all information bearing on these hypotheses; (3) to select from the set of competing hypotheses the one hypothesis best supported, or the set of those hypotheses equally well supported, by the data; (4) to evaluate in some way the degree of confidence which can be placed in the rejection of the hypotheses not so selected." ("The Econometric Approach to Business Fluctuation," in *Papers and Proceedings, American Economic Review*, Vol. XXXIX, No. 3, 1949, p. 70.)

However, the number of possible conjectures to which available data "may conceivably make an answer possible" can be large for many problems in economics, such as that of business fluctuations; moreover, often progress requires the gathering of additional data, or the correction of available data, in order to test a conjecture. In general, all too often we find a situation in which the available data are inadequate for choosing among the conjectures deemed plausible by some economists. (For a discussion of the types of error found in economic data, see Oskar Morgenstern, *On the Accuracy of Economic Observations*, rev. ed., Princeton, Princeton University Press, 1963.)

Some econometricians have been highly critical of much work done in their field and object to the use of dubious assumptions. Leontief says:

> "[Econometrics] can be in general characterized as an attempt to compensate for the glaring weakness of the data base available to us by the widest possible use of more and more sophisticated statistical techniques. . . . However, like the economic models they are supposed to implement, the validity of these statistical tools depends itself on the acceptance of certain convenient assumptions pertaining to stochastic properties of the phenomena which the particular models are intended to explain; assumptions that can be seldom verified." ("Theoretical Assumptions and Nonobserved Facts," pp. 2-3.)

Much work in econometrics, then, rather than leading to warranted assertions, is subject to the difficulties of the mathematical models discussed earlier.

Experimental economics. An assortment of inquiries into economic problems has been described as experimental, including work in testing aspects of game theory and utility theory; work in computer simulation; and "real world" experiments. (For numerous examples, see Thomas H. Naylor, "Experimental Economics Revisited," *Journal of Political Economy*, Vol. 80, 1972.)

The critics of such work argue that the controlled laboratory situations are unlike typical economic behavior outside the laboratory, that the alleged control of variables in many experiments is defective, that typical important economic problems involve so many variables that the requisite control for an experiment is not possible, etc.; and that as a consequence the results of experimentation have only a limited significance. Barry Castro and Kenneth Weingarten have argued that the use of infrahuman subjects would make possible the necessary controls and that animal experiments may provide useful information concerning human behavior. ("Toward Experimental Economics," *Journal of Political Economy*, Vol. 78, 1970.)

Although we do not oppose the use of experimental procedures anywhere they may prove useful, some defenders of experiments seem to overemphasize the role of experimentation in scientific inquiry. In many instances (with astronomy being the classic instance) warranted assertions may be developed through the measurement of changes—changes that the investigator can neither initiate nor otherwise control in the manner of a laboratory experimenter.

4. RESULTS ACHIEVED

The body of literature in economics is vast. Economists appear to discuss earlier work in their field more frequently than do inquirers in many other behavioral fields, and the current output of literature is sizable. In 1969 an estimate was made that from 1,300 to 1,500 books "associated in some significant sense with the discipline" are published each year, and more than 5,000 major articles. ("Editor's Note," *Journal of Economic Literature*, Vol. VII, 1969, p. iii.)

Numerous classifications of the major subfields of economics can be found. The American Economic Association currently uses the following major divisions for classifying the economic literature:

General economics; Theory; History; Systems
Economic growth; Development; Planning; Fluctuations
Economic statistics
Monetary and fiscal theory and institutions
International economics
Administration; Business finance; Marketing; Accounting
Industrial organization; Technological change; Industry studies
Agriculture; Natural resources
Manpower; Labor; Population
Welfare programs; Consumer economics; Urban and regional economies

In view of the extensive literature in economics, our comments in this Section must be highly selective. We shall first sketch some of the historical developments in the field, then consider some overall assessments of what has been achieved, and finally look at some of the recent work in economics.

Some Historical Aspects

Although discussions of economic topics can be found in much earlier writers, the French physiocrats (Francois Quesnay and disciples) and Adam Smith usually are regarded as the founders of economics as a specialized field of inquiry. The economic systems discussed by the classical economists were technologically undeveloped, primarily agrarian, and strongly controlled by national governments. The early economists offered both descriptions of observed connections among events and policy recommendations. They devoted much attention to the harmful effects of government intervention and to a defense of "laissez-faire" as the most effective way of promoting economic efficiency and development. They emphasized the benefits of free trade and discussed the advantages of a domestic and international exchange system based on a gold standard.

The classical economists attempted to formulate a system of fundamental and consistent premises from which further conclusions could be developed logically. Thomas Malthus was one of the first to conjecture about the causes of economic crises. In his *An Essay on the Principles of Population* (1798), he argued that the population tends to increase geometrically and the means of subsistence only arithmetically, and he concluded that most of the population therefore must live in misery at the bare subsistence level, unless disasters such as war and famine, or moral restraints such as deferred marriage, reduced the population. Jean Baptiste Say (*Treatise on Political Economy*, 1800) argued that for the economy as a whole, the demand for commodities and the supply of commodities are different aspects of the same process (supply creates its own demand). David Ricardo (*Principles of Political Economy and Taxation*, 1817) discussed the distribution of wealth and income within a society, and formulated a "labor theory of value" in which labor was viewed as the major factor underlying exchange value. John Stuart Mill (*Principles of Political Economy*, 1848) argued that the laws of distribution are not as immutable as the laws of production, and in general his conclusions suggested that the pessimistic consequences envisaged in Malthus' "dismal science" would not occur.

Although Karl Marx argued that capitalism would be superseded by socialism, which would then develop into communism, and thus rejected a basic tenet of classical economics, he adopted the general procedures of inquiry followed by the classical economists. His use of Ricardo's labor theory of value is an example of the way Marx attempted both to use and to transform traditional economics. According to Marx, workers produce more value than they receive in wages. The surplus value thus created is appropriated by the capitalists in the form of profits and becomes the basis for capital accumulation. Marx argued that cyclical depressions were intrinsic to the capitalist system and that inherent contradictions would lead to the ultimate overthrow of capitalism and the establishment of a classless society. His *Capital* (1867) also excoriates the working and living conditions of the laborers of the time. Marx transformed Hegel's dialectical idealism into a dialectical materialism in which the course of human social development is largely determined by the economic and material conditions of life.

Henry George (*Progress and Poverty*, 1879; *The Science of Political Economy*, 1897) had a considerable impact on the social thought of his time, although he was far less influential on academic economists. George criticized conventional economists for the looseness with which they used technical terminology. He was one of the foremost proponents of free competition or voluntary cooperation, and urged equality of opportunity for all and special privileges for none. George supported a "single tax" on land values that was intended to force those holding such special privileges to pay the competitive market price for their privileges and thus to help restore equality of opportunity.

Several neoclassical schools of economics developed in the period from 1870-1914, including the Jevonsian school (William S. Jevons, Philip Wicksteed, F.Y. Edgeworth), the Austrian school (Karl Menger, Eugen von Böhm-Bawerk, Frederick von Wieser), the Walrasian school (Leon Walras, Vilfredo Pareto), the Scandinavian school (Knut Wicksell, Gustav Cassell), and the American school (John B. Clark, Irving Fisher, Frank Fetter). The neoclassicists tended to make economic inquiry even more deductive than it had been.

A major figure is Alfred Marshall, who attempted to extend the range of economics beyond the study of wealth and to unify the economic "theories" of his time. His *The Principles of Economics* (New York, Macmillan, 1890) contains a detailed discussion of the business and industrial developments characteristic of the industrial revolution. He extended and coordinated the prevailing views about supply and demand, marginal utility, general equilibrium, and substitution and marginal productivity; and emphasized the diagrammatic method of presenting economic analyses. His work served as a basis for much subsequent economic inquiry.

The early American institutionalists, such as Thorstein Veblen and John R. Commons, in many respects strongly opposed the doctrines of the traditional economists (see our prior discussion in Section 3). The institutionalists adopted a point of view that was emerging, although not always coherently, in many fields; their work has similarities to that of Roscoe Pound in jurisprudence, J.H. Robinson in history, William F. Ogburn in sociology, Arthur F. Bentley in political science, and Charles S. Peirce, William James, and John Dewey in philosophy. Attention was directed to the observable behavior of social man, rather than to the deductive elaboration of assumed "truths." An evolutionary perspective was often adopted, and "human nature" was viewed not as fixed, but as molded by changing sociocultural circumstances.

John Maynard Keynes' views have been heralded as revolutionary and as initiating a "new economics," but his procedures of inquiry were basically those associated with the traditional economists. Although many of the earlier writers had been concerned with "macroeconomic" problems, Keynes believed that none of them had dealt adequately with overall or aggregate output, income, and levels of employment. Basic to his system is the notion of *gross national product* (the total value of all final goods and services). Although Keynes has been severely criticized by those who object both to his procedures of inquiry and his conclusions, his work has been extremely influential on recent economists. (For one such criticism, see W.H. Hutt, *Keynesianism–Retrospect and Prospect*, Chicago, Regnery, 1963.) Probably it is no exaggeration to say that most English-language economics taught today is at least partly Keynesian.

Overall Assessments of the Field

There are strongly divergent assessments of the merits of what has been accomplished in the field of economics. Both economists and non-economists often view economics as the most advanced of the behavioral fields; Samuelson's statement that economics is "the queen of the social sciences" is typical of those assessments. (*Economics*, p. 1.) According to F.A. Olafson:

> "Among the sciences generally, physics of course offers the supreme example of success in the discovery of such laws [predictable regularities that hold for all such phenomena wherever and whenever encountered]; and it is a debatable question whether *any* of the social sciences have really discovered any comparable regularities. Nevertheless, there is wide agreement that economics has progressed further in this direction than any other social science. This means that economists have been more successful in isolating those features of human behavior that are relevant and fruitful for the kinds of prediction they are interested in making than have the other social scientists. . . ." ("Some Observations on Area Study Programs," *American Behavioral Scientist*, Vol. 8, 1964, p. 12.)

The wealth of "theory" (i.e., elaborate conjectures) developed by economists often is offered as proof of the progress that has been made. The mathematical conjectures so prominent in recent years perhaps are the most admired of such conjectures, although even the earlier deductive procedures are sometimes suggested as models for work in other behavioral areas. (Arnold Rose, for example, urged his fellow sociologists to copy the method "so brilliantly employed" by the classical economists. See his "A Deductive Ideal-Type Method," *American Journal of Sociology*, Vol. 56, 1950.)

Other commentators take a much more critical view:

> "The apparatus of economics is very flexible: without breaking the rules of the profession—by being illogical or even by denying the validity of the traditional theory—a sufficiently clever person can reach *any* conclusion he wishes on any *real* problem (in contrast to formal problems). In general there is no position. . .which cannot be reached by a competent use of respectable economic theory." (George Stigler, "Politics of Political Economists," *Quarterly Journal of Economics*, Vol. LXXIII, 1959, p. 531.)

> "That economists frequently do not agree has become so commonplace that some economists no longer seem to be troubled by the suggestion that such a state of affairs is scandalous. That many economists do agree on certain analyses and conclusions is equally scandalous from the viewpoint of modern science, however, because that agreement rests on methods of inquiry that have been found unreliable and have been discarded by capable scientists." (E.C. Harwood, *op. cit.*, p. 3.)

> "The achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous. . . . It is an unsatisfactory and slightly dishonest state of affairs." (F.H. Hahn, "Some Adjustment Problems," *Econometrica*, Vol. 38, 1970, pp. 1-2.)

> "Economics today rides the crest of intellectual respectability and popular acclaim. . . . And yet an uneasy feeling about the present state of our discipline has been growing in some of us. . . . The uneasiness. . .is caused. . .by the palpable *inadequacy* of the scientific means with which they try to solve [practical problems]. The weak and all too slowly growing empirical foundation clearly cannot support the proliferating superstructure of pure, or should I say, speculative economic theory." (Wassily Leontief, "Theoretical Assumptions and Nonobserved Facts," p. 1.)

Some Recent Work

Economic inquiry sometimes is divided into *microeconomics*, the study of the economic behavior of firms and households, and *macroeconomics*, the study of the overall behavior of economic systems. Although the usefulness of the distinction is open to question, we follow that division here for convenience.

Microeconomics. Workers in microeconomics frequently assume that the objective of firms is to maximize profits, and the objective of households is to maximize satisfaction. Given those assumptions, analyses are made of how producers and consumers presumably will behave under specified sets of circumstances. Considerable work since the time of Marshall has been devoted to conjectures about equilibria. For example, the neoclassical economists held that as more of a particular commodity was possessed, the smaller the "utility" yielded by gaining an additional unit of that commodity; they argued that an equilibrium for a consumer is achieved when the ratios of the marginal utility to unit cost are equal for all the goods purchased. Many other possible equilibria (not necessarily relying on marginal utility notions) also have been discussed, for firms and for the whole system, with a key notion being that once an equilibrium is established, the situation tends to remain stable unless disturbed from the outside.

The distance between the usual conjectures about equilibria and the observation of behavior is considerable. To illustrate, after claiming that the "rigorous foundation of the theory of general competitive equilibrium is one of the major achievements of economic theory during the past fifteen years," Solow discusses several questions not answered by the "theory," and goes on to say:

> ". . .the theory of general economic equilibrium is far from complete. Indeed, only sketchy results are known, and active research is being carried on by a small but very talented group of theoretical economists. Their object is not only to find more complete answers to the questions already mentioned, but also to extend the theory in still other ways. For example, it is of the greatest importance to extend the theory of general equilibrium to cover market structures other than perfect competition. . . . Moreover, the theory is almost entirely static; even when it allows formally for the passage of time, it does so by assimilating the multiperiod economy to the one-period case. . . . The theory. . . tends to ignore such phenomena as the holding of quite different expectations about the future by different households and firms, and the speculative activities to which those different expectations give rise." (Solow, *op. cit.*, p. 33, p. 35.)

And Hahn, who believes that the "technically best work in the last twenty years" has been in the study of equilibria, also says that the "study of equilibria alone is of no help in positive economic analysis," and that "to discuss and analyze how the economy works *it may be necessary to go and look.*" (Hahn, *op. cit.*, pp. 11-12, p. 1. Italics added.)

Price theory is considered by some to be the major part of microeconomics, or synonymous with it, for many of the conjectures about supply and demand start from assumptions concerning the behavior of households and firms. Something of the extensive nature of the work economists have done on prices, the complex character of the conjectures that have been made, and the type of conflicting conclusions arrived at, can be gained by a study of Arthur W. Marget's two-volume work, *The Theory of Prices* (New York, Prentice-Hall, 1938, 1942.)

The foregoing should not be understood as denying that "empirical" work is done in microeconomics; however, the dominant tendency has been to begin with elaborate conjectures and then to test them "empirically," if such testing is done at all.

Macroeconomics. According to James Tobin:

> "Macroeconomics concerns the determinants of the performance of entire economies: nations, groups of nations, the whole world. The theoretical concepts and statistical measures involved are generally economy-wide aggregates or averages, such as national income, total employment, or a national cost-of-living index. The objective is to explain ups and downs of these magnitudes and their interrelations. . . . Macroeconomics is based on the faith that economies are subject to laws of motion which are largely independent of their internal structure." ("Macroeconomics," in Ruggles, *op. cit.*, p. 44.)

Although much of the work of the classical economists, as well as of more recent inquirers, was concerned with large-scale trends, what today is called "macroeconomics" has developed during the last forty years or so. Keynes' *General Theory* (1936) was probably the most influential single work in the development of explicit macroeconomic model-building. Keynes wanted to develop a "theory" that would deal with the entire economic system in terms of a few large quantities, such as national income, stock and flow of money, consumption, investment, savings and income, employment, wages, and general prices. Keynes rejected the widely-held view that voluntary unemployment was a temporary disequilibrium; he saw such unemployment as a possible stable state of the system. He also rejected Say's Law, and argued that government intervention often could stimulate economic activity in periods of recession or depression.

Government intervention, probably because of the influence of Keynes, sometimes is taken as a key aspect of macroeconomic inquiry. For example, in his recent textbook Willis L. Peterson says:

> "Macroeconomics. . .is concerned mainly with economic aggregates, or the economy as a whole. The two major problem areas of macroeconomics are unemployment and inflation. . . . [These] are problems that the individual has virtually no control over. Rather the cause and/or solutions to these problems lie in the realm of government action; action which affects the entire economy." (*Principles of Economics: Micro*, Homewood, Richard D. Irwin, 1971, p. 1.)

However, some economists who work on the problems of inflating and the business cycle do not believe that government intervention helps to solve the social difficulties involved. In general, the macro-micro distinction does not seem useful except as a rough indication of subject matter areas, and many problems in economics may require investigation in both the macro and the micro areas.

Much of the recent work done by economists, then, consists in the building of a variety of models. The prestige of such work is so great among professional economists that we have given much space in this Chapter to that topic. On the other hand, considerable work also has been done that has a much firmer basis in observation; e.g., the work of the National Bureau of Economic Research in gathering statistical data, measuring economic changes, and developing economic indicators. Perhaps such scientific work will become even more prominent in the future; the two recent presidential addresses quoted earlier (Leontief's to the American Economic Association, and Hahn's to the Econometric Society) raise questions about the usefulness of "pure theory."

5. CONTEMPORARY CONTROVERSY

We have already discussed a number of controversies among economists, particularly those concerned with the procedures of inquiry. Basic to many of the methodological controversies is a disagreement about the stage of inquiry at which elaborate conjectures can be developed most usefully. The latter disagreement comes out clearly in Tjalling Koopmans' criticism of the work of Mitchell and Burns on business cycles.

Mitchell argued that although conjectures or hypotheses are used "continuously" to help ascertain which activities and relations among them are worth investigating, inquiry should begin with the observation of behavior in a problem situation, not with a "theory." Existing "theories," he said, often can be neither confirmed nor refuted by the available data, and many of those "theories" were developed by individuals who had "sadly incomplete" or "badly distorted" information about the relevant facts. (Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles*, New York, National Bureau of Economic Research, 1946, pp. 8-10.)

Koopmans regarded such work as "measurement without theory" (the title of his long review of the Burns-Mitchell book in *Review of Economics and Statistics*, Vol. 29, 1947). Koopmans argued in favor of combining "a priori knowledge or assumption with observation," and said:

> ". . .the approach of the National Bureau of Economic Research to the empirical study of business cycles, developed by Mitchell, Burns, and their associates, emphasizes the number and wide coverage of observations to the virtual exclusion of explicit a priori specification. . ." (*Three Essays on the State of Economic Science*, New York, McGraw-Hill, 1957, p. 199.)

According to Edmond Malinvaud:

> ". . .Koopmans clearly thought that economic science had little need for extensive contributions to

> the 'Kepler stage' of inquiry, in which empirical regularities are searched in the hope that they will suggest hypotheses. According to him we are now in the 'Newton stage' in which inference must be based on rigorously specified models." ("The Scientific Papers of Tjalling C. Koopmans: A Review Article," *Journal of Economic Literature*, Vol. X, 1972, p. 801.)

Presumably those arguing for Newtonian procedures in economics have not overlooked the obstacles to further physical inquiry set up by Newtonian absolutism, but they may overlook the connection between such absolutisms and the development of plausible models on the basis of insufficient evidence.

Within the group of economists who favor "scientific" procedures of inquiry, then, we find a marked disagreement about the usefulness of *a priori* models.* Other economists, however, view economics primarily as an *art*, rather than as a science. And some, relying on the "nomothetic-idiographic" distinction, argue that the objective of economic inquiry is not the development of warranted *generalizations* about behavior, but the development of "knowledge" about *particular* historical situations.

Sometimes several of those views are combined. Sidney Schoeffler, for example, says that "economics is not a nomothetic empirical science." Part of economics is deductive and belongs to mathematics and logic, part is idiographic and describes particular historical facts, but "most important, by far, *economics* is an art." (*The Failure of Economics: A Diagnostic Study*, Cambridge, Harvard University Press, 1955, pp. 155-156.)

There also has been considerable discussion concerning the "role of value judgments" in economics and the distinction between "positive" and "normative" economics. The earlier economists tended to differentiate sharply between positive economics (the description of "what is") and normative economics ("what ought-to-be") and maintained that positive economics is "objective," "neutral," "disinterested," etc. Many recent economists have adopted the same view. Milton Friedman, for example, says:

> "Positive economics is in principle independent of any particular ethical position or normative judgments. . . . Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. . . . In short, positive economics is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences." (*Essays in Positive Economics*, Chicago, University of Chicago Press, 1953, p. 4.)

Other economists argue that moral judgments are inescapable even in descriptive economics. Joan Robinson, for example, says:

> "But it is not possible to describe a *system* without moral judgments creeping in. For to look at a system from the outside implies that it is not the only possible system; in describing it we compare it (openly or tacitly) with other actual or imagined systems. Differences imply choices, and choices imply judgment. We cannot escape from making judgments and the judgments that we make arise from the ethical preconceptions that have soaked into our view of life and are somehow printed in our brains." (*Economic Philosophy*, Chicago, Aldine, 1962, p. 14.)

Such controversies are complicated by confusions and disagreements as to what words such as "moral," "ethical," and "value" designate. Also, without denying that often what a human believes to be factual is strongly influenced by preconceptions of which that person is unaware, or only partially aware, the successes of scientific inquiry amply show that warranted assertions often can be developed that eliminate or minimize the effects of those initial preconceptions. Other procedures of inquiry often reinforce the initial preconceptions, and the agreement reached through the use of those procedures may signify little else than that the preconceptions have been elaborated logically. (For a detailed discussion of the range of economists' views on this topic, see T.W. Hutchison, *'Positive' Economics and Policy Objectives*, Cambridge, Harvard University Press, 1964.)

In recent years work in the field of economics, like that in most other behavioral fields, has been criticized by members of the "new left" and other radicals. Establishment economics is said to be preoccupied with trivial problems, rather than with problems such as the unequal distribution of income, racism, and militarism; to lend support to imperialism; to have a "fixation" on an ever-increasing Gross National Product, rather than to be concerned with crucial ecological problems; etc. The Union for Radical Political Economics, organized by graduate students and younger economists, has a sizable membership. Some of the work by the radical economists reflects a Marxist orientation, while some is anarchistic. Although a bitter attack has been mounted both on the importance of many typical problems selected by economists for inquiry and on many of the typical conclusions they have reached, the radical economists tend to adopt the same procedures of inquiry that their opponents use. (For a sampling of the radical literature, see John G. Gurley, "The State of Political Economics," in *Papers and Proceedings, American Economic Review*, Vol. LXI, No. 2, 1971; "Symposium: Economics of the New Left," by G.L. Bach, Stephen Hymer and Frank Roosevelt, Paul Sweezy, and Assar Lindbeck, *The Quarterly Journal of Economics*, Vol. LXXVI, 1972; Angus Black, *A Radical's Guide to Economic Reality*, New York, Holt, Rinehart, and Winston, 1970; and Paul A. Baran and Paul M. Sweezy, *Monopoly Capital*, New York, Monthly Review Press, 1966.)

A large number of other controversies could be mentioned, ranging from technical disagreements to disagreements about fundamental economic policy. Almost every shade of opinion can be found concerning the extent to which governmental intervention can improve economic performance, the relative merits of central vs. individual planning, and related matters. At one time, most economists agreed that maximum individual freedom would yield the greatest economic development and the most efficient allocation of resources; more recently many of the most influential economists have argued that considerable governmental regulation and control is necessary to achieve those objectives. Many of the more technical disagreements

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*Mention also may be made of Milton Friedman's well-known view that conjectures may be "valid" even if based on "unreal" assumptions that run counter to well-established findings; "the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience." (Ch. I of *Essays in Positive Economics*, Chicago, University of Chicago Press, 1953. The quotation is from pp. 8-9.)

among economists seem to stem from differences of opinion about the efficacy of "free markets"; Hahn claims that the "most intellectually exciting question of our subject remains: is it true that the pursuit of private interest produces not chaos but coherence, and if so, how is it done?" (Hahn, *op. cit.*, p. 12.)

Although sharply divergent opinions about social objectives sometimes may be involved, many of the controversies just mentioned apparently are based primarily on disagreements about the probable consequences of alternative courses of action. Disputes about probable consequences may continue until modern procedures of inquiry are adopted; i.e., until application of the procedures associated with the Galilean revolution yields warranted assertions.

6. TERMINOLOGICAL PROBLEMS

A major obstacle to the development of economic inquiry is the lack of useful naming such as that found in the physical and physiological sciences. Basic terminology in economics often is applied inconsistently and incoherently. The existing confused terminology is described in Chapter II of E.C. Harwood's *Useful Economics* (Great Barrington, American Institute for Economic Research, 1970 edition). Although we believe that many of the suggestions about terminology in L.M. Fraser's *Economic Thought and Language* (London, Adam and Charles Black, 1947) are far from meeting the requirements of scientific specification, his book illustrates in detail many of the ways in which economists have attempted to use key names and the resulting difficulties.

In what follows, we have selected examples of *some* of the major types of semantic problems encountered in economics:

(1) Often a single word is used confusingly to name diverse aspects or phases of a situation. "Money," for example, sometimes designates only the money commodity (gold) and representative paper (such as the gold certificates formerly in circulation); sometimes currency plus demand deposits (checking accounts); sometimes saving deposits in addition to currency and demand deposits; and sometimes other things also. Assertions about "money" that are warranted for some of those uses are not warranted for others. (For a further discussion of such matters and the recommended use of "purchasing media," rather than "money," to designate the total of hand-to-hand currency plus checking accounts immediately available to the public, see E.C. Harwood, *Cause and Control of the Business Cycle*, 9th ed., Great Barrington, American Institute for Economic Research, 1971, pp. 4-6.)

(2) Economists sometimes use words in a special sense, at the same time relying on many of the usual connotations of those words in order to make their argument plausible. For example, in discussing the work of von Neumann and Morgenstern, Kenneth Arrow (a recent Nobel Laureate in Economics) says:

> "The point here, broadly speaking, is that, once a machinery for making social choices from individual tastes is established, individuals will find it profitable, from a rational point of view, *to misrepresent their tastes by their actions*.... Thus, in an electoral system based on plurality voting, it is notorious that an individual who really favors a minor party candidate will frequently vote for the less undesirable of the major party candidates rather than 'throw away his vote.'" (*Social Choice and Individual Values*, 2nd ed., New York, Wiley, 1963, p. 7. Italics added.)

Arrow goes on to say that the problem is therefore to find rules for a game so that individuals "will actually express their true tastes even when they are acting rationally."

To describe a situation in which a person votes for the least undesirable of the only candidates having a chance of getting elected as a *misrepresentation* of a "true taste" (whatever that may be) seems a confusing use of language, unless one assumes that voting for a candidate somehow signifies approving that candidate above all others. A common situation in life is desiring something that is presently unattainable; so to allocate one's resources in order to get the best that is attainable hardly is a misrepresentation of one's ideal.

(3) At times economists mistakenly assume that something exists in behavior corresponding to some part of the model used by the economist. Many uses of "utility" reflect such an assumption. For example, economic behavior often is construed as an attempt to maximize something; for consumers, "utility" is postulated as that something. Maximization models are then developed in which numerical measures can be manipulated and solutions arrived at; certain of those measures are labeled as utilities and are assumed to correspond to something found in observable behavior. But observation of behavior rarely reveals the purported utilities.

Other economists have viewed utilities not as something found in economic behavior, but as "logical entities" useful for achieving logical closure of a model. Typically such conjectures are developed so far in advance of the relevant observations that they are not useful. (For a brief history of the notion of *utility*, see Jerome Rothenberg, "Values and Value Theory in Economics," in Sherman Roy Krupp, ed., *The Structure of Economic Science*, Englewood Cliffs, Prentice-Hall, 1966.)

(4) Many economists use the self-actional language long outmoded in scientific inquiry and emphasize the role of an Actor spurred on by internal forces. For example, Rothenberg says:

> "The concept of utility here has been a useful buffer between the action of choice and the supposed psychological ground of this action. By being able to speak of maximizing utility, the economist has not had to say that individuals try to maximize gratification, or satisfaction, or pleasure, or happiness, or virtue, etc., each one of which would seem to be making an empirical commitment in the field of psychology. Utility seems philosophically neutral, while the others seem to assert something about the substantive quality of the ultimate inner goad—if indeed it is unitary." (*Ibid.*, p. 227.)

Although there may be some gain in refraining from discussions about the "substantive quality" of the presumed goad, to assume that there is an inner maximizing goad or goads is hardly neutral; self-actional rather than transactional procedures are implied.

Economists who want to be "scientific" often argue for the use of special methods to handle "inner states." George Katona, for example, says:

> "Behavioral science derives from interest in and concern with factors that shape the behavior of

people. . . . But human beings have 'images' of the future which influence their behavior and which, since they are endowed with language, they can communicate to others. Access to such intervening variables as motives and expectations requires the use of a methodology which differs somewhat from that of other sciences." (Letter to the Editor, *Behavioral Science*, Vol. 7, 1962, p. 483.)

To sum up, the terminology used by economists in discussing both technical matters within the field and general procedural matters often hinders communication. The American Institute for Economic Research has long been concerned about improving that situation. See "Toward More Precise Economic Naming" (*Research Reports*, July 8, 1968) and the *Economic Naming* sections in subsequent issues.

7. COMMENT AND EVALUATION

Many historically influential economic conjectures were developed at a time when the measurements of change necessary for a thorough testing of those conjectures were not available. Instead of attempting to improve the observational basis from which conjectures could be made and restricting their conjectures to those that could be tested adequately, many of the early economists applied methods that could be followed without such testing. The often quoted statement by John Stuart Mill illustrates the extent to which the quest for certainty was sometimes pursued: "Happily, there is nothing in the laws of Value which remains for the present or any future writer to clear up; the theory of the subject is complete. . ." (*Principles of Political Economy*, Book III, Ch. I, Sec. 1, 1948; quoted from Vol. I of the 1884 edition, New York, D. Appleton, pp. 536-537.) Although some later economists were less inclined to make assertions as far-reaching as Mill's, they often exhibited great confidence in the soundness of various economic conclusions that no longer can be considered warranted assertions.

However unsound economic views may be, influential political groups sometimes adopt them as "articles of faith." For example, in his autobiography John R. Commons tells of his employment by the Democratic party in 1900. The Democrats, influenced by the views of W.J. Bryan and others, were convinced that under the gold standard the quantity of goods would increase faster than the quantity of money, and prices therefore would fall. Commons was hired to construct and publish a weekly price index number. Unfortunately for the Democrats, the index number stopped falling in the middle of August and began to rise in September. Commons then was promptly discharged, although his contract had seven months to run. (John R. Commons, *Myself*, New York, Macmillan, 1934, p. 66.)

In the 1930's many economists maintained that the United States economy had reached the apogee of its growth; in the period just after World War II, many predicted an immediate depression; and in late 1969, some economists predicted without qualification that there would be no recession in 1970; but all these assertions proved to be erroneous. This is not to deny, of course, that the predictions made by economists often are accurate; indeed, at a given moment reputable economists may make so many conflicting forecasts that one is almost certain to be correct. (To give one illustration, in December of 1969 Milton Friedman said that a recession on the order of that in 1960 "seems to be in the cards" for 1970; Raymond J. Saulnier said there was a 50-50 chance of a recession; and Pierre A. Rinfret said that "There ain't gonna be no recession in 1970, period." [*New York Times*, issues of Dec. 10 and Dec. 17, 1969.])

The following seems a fair statement about many economists: Their preferred conjectures or "models" (whether verbal, mathematical, econometric, computer, game theory, etc.) seem so plausible that they believe economic behavior must be describable at least approximately by their conjectures, and many predictions and policy recommendations are made in good faith on the basis of the conjectures. When events prove that the predictions or recommendations are mistaken, modifications may be made in the conjectures but confidence in the usefulness of extensive conjectures or hypotheses (sometimes called "theory") is retained. (For a nontechnical account of both the erroneous predictions often made and the continuing trust exhibited in the usefulness of the procedures of inquiry underlying the predictions, see "Bad Year for Econometrics," *Business Week*, Dec. 20, 1969.)

We suggest that the difficulty is fundamental and consists in the development of conjectures far in advance of the relevant observations and measurements. We recognize that frequently the problems of men-in-society are so critical that the temptation to arrive at "solutions" before the relevant facts are available is almost overwhelming. However, there is no dearth of instances in which presumed "short cuts" to useful warranted assertions have worsened the original situation, and the time spent on mistaken "short cuts" cannot be used for inquiry that eventually may solve the problems adequately.

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INTERNATIONAL ECONOMIC REVIEW
JOURNAL OF ECONOMIC ABSTRACTS (superseded by JOURNAL OF ECONOMIC LITERATURE)
JOURNAL OF ECONOMIC EDUCATION
JOURNAL OF ECONOMIC HISTORY
JOURNAL OF ECONOMIC THEORY
JOURNAL OF FARM ECONOMICS
JOURNAL OF FINANCE
JOURNAL OF LAW AND ECONOMICS
JOURNAL OF MONEY, CREDIT AND BANKING
JOURNAL OF POLITICAL ECONOMY
JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION
LAND ECONOMICS
MONTHLY LABOR REVIEW
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SOUTHERN ECONOMIC JOURNAL
WESTERN ECONOMIC JOURNAL

VII.

HISTORY*

1. WORKING DESCRIPTION OF THE FIELD

HISTORIANS inquire into what happened in the past, with emphasis on developmental changes, the sequence of significant events, the circumstances from which the events developed, and the connections among those events. Historians make considerable use of the findings and methods of inquiry of many other fields, including political science, economics, psychology, sociology, demography, anthropology, archaeology, and geography.

2. OTHER DESCRIPTIONS OF THE FIELD

"History" has been used to name many things, the most pertinent of which for our purposes are: (a) a tale or story; (b) a record of presumed significant happenings about a person, institution, or problem (as in case histories); (c) a systematic account of sequences of presumed significant events together with an account of what are believed to be the causes of those events; (d) the events themselves, as differentiated from attempted descriptions of those events; (e) reifications of the total past of everything, or of some portion of the past, as when History is said to demand or to produce something.

The descriptions of the field of history found in the literature vary widely. Some representative descriptions follow.

Sometimes history is viewed as a group recollection of significant events. G.J. Renier, for example, regards history as figuratively "the memory of societies." As custodian of social memory, the historian helps to answer "society's urgent demand for specific points of comparison taken from past experience." (*History: Its Purpose and Method*, London, George Allen & Unwin, 1950, pp. 22-24.) And Carl Becker says: "'History is the memory of things said and done.' This is a definition that reduces history to its lowest terms, and yet includes everything that is essential to understanding what it really is." (*Everyman His Own Historian*, New York, Appleton-Century-Crofts, 1935, p. 235.)

Often historians believe that the main distinctive feature of historical inquiry is a focus on particular or individual events. Geoffrey Elton, for example, says: "History is concerned with all those human sayings, thoughts, deeds, and sufferings which occurred in the past and have left present deposit; and it deals with them from the point of view of happening, change, and the particular." (*The Practice of History*, Sydney, Sydney University Press, 1967, p. 12.) Frequently this emphasis on the particular is taken as a basic distinction between historical inquiry and scientific inquiry (see Section on Contemporary Controversy). The philosopher Morris R. Cohen says:

> "Now history is concerned with establishing specific events that occurred at a definite time and place, whereas the facts or laws which general physical science seeks to establish deal with repeatable elements and assert that whenever and wherever A then B. . . . The difference, then, between history and general science in this respect is unmistakable." (*The Meaning of Human History*, LaSalle, Open Court, 1947, pp. 36-37.)

Cohen continues:

> "What is distinctive, then, about human history, is not its material, which is identical with the material of the social sciences, nor the critical apparatus that is utilized to search out this material and consists primarily of hypotheses borrowed from the sciences. What is distinctive is rather the focus or perspective which makes description or understanding of individual happenings in time and place central." (*Ibid.*, pp. 40-41.)

Many historians emphasize the development of "explanations" of the ordering of events. The 1954 *Report* of the Committee on Historiography of the Social Science Research Council says:

> ". . .an essential problem of history is the description and explanation of human activity through time. . . . [A] chief task of the historian is to ascertain what has happened, to identify events in sequence, to analyze interrelations among those events, and to discover how and why they occurred in a given order." (*The Social Sciences in Historical Study*, Bulletin No. 64, New York, Social Science Research Council, 1954, p. 106.)

Some historians believe the past should be studied for its own sake, while others believe that historical inquiry is important for the illumination it can provide about contemporary or future events. According to the 1946 *Report* of the Committee on Historiography:

> "The historian. . .aims to compose accurate accounts and analyses of selected portions of the past. From these accounts and analyses, or from the original sources themselves, he endeavors to reach generalizations that appear to be valid. On the basis of his knowledge he also seeks to provide credible explanations of the development of contemporary events, thoughts, manners, and institutions. (*Theory and Practice in Historical Study*, Bulletin No. 54, New York, Social Science Research Council, 1946, p. 134.)

And Curtis P. Nettels says:

> "History is man's guide to action in the present and future. And such action is certain to be most constructive when it is informed by an understanding of the problems and conditions which, having emerged slowly from the past, mold and limit the activities of today and tomorrow. Those who know the circumstances of their country's development and who understand the elements of its civilization

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*This chapter, as it appeared in the first edition, was critically reviewed by Richard T. Vann, who made many suggestions for improvements. His efforts are appreciated; however, he should not be understood as endorsing the point of view of the authors.

> will be the ones best qualified to meet present issues with decision, intelligence, and economy of effort. (*The Roots of American Civilization*, New York, Appleton-Century-Crofts, 1938, p. xi.)

What is often called "universal" or "speculative" history is an attempt to discover general patterns or laws applicable to the history of human society as a whole. Arnold Toynbee is the most notable recent representative of that point of view. He attempted to describe the entire course of human history by distinguishing 21 different civilizations and considering their efforts to respond effectively to challenges stemming from their social or natural environment. (*A Study of History*, London, Oxford University Press, 11 vols., 1934-1959.) Most contemporary professional historians view universal history with great disfavor, although Geoffrey Barraclough has strongly criticized his colleagues for not giving more attention to what history as a whole is all about: "The failure of the historian to provide an interpretation of history, to say what it is all about, is another example of the notorious *trahison des clercs*, of the refusal of the specialist to live up to his work." (*History in a Changing World*, Oxford, Blackwell, 1955, p. 222.)

3. METHODS AND TYPES OF INQUIRY

Historians have often used both some of the methods characteristic of writers in the field of literature and some of the methods and viewpoints of behavioral scientists. For a considerable period of time there have been those who urge completely scientific procedures of inquiry into historical subject matters; within that group, however, the label "science" has been used in many different ways. In this Section some of the major ways in which historical subject matter has been investigated are described. Some of those ways are fundamentally nonscientific, while others are compatible with modern scientific procedures of inquiry even though those procedures may not be widely used.

Narrative Emphases

Narrative elements are found in all histories, but in the following kinds of historical inquiry the narrative approach is paramount.

(1) *History as Imaginative Re-enactment.* Many historians have as their chief aim the reconstruction of a past sequence of events through an empathic understanding of the thoughts and motives of the historical persons involved. According to John Higham:

> "No amount of scientific analysis or synthesis can take the place of that crucial act of human empathy by which the historian identifies himself with another time and place, re-enacting the thoughts and reliving the experience of people remote from himself. Thus he tries to catch the distinctive resonance of a person, a situation, and an age, as it manifests itself amid the other phenomena among which it arises and into which it passes." (John Higham, with Leonard Krieger and Felix Gilbert, *History*, Humanistic Scholarship in America Series, Englewood Cliffs, Prentice-Hall, 1965, p. 143.)

This type of historian attempts to make his description of the motives of historical agents plausible to modern readers and to give them the "feel" of what it was like to live through such events. Such historians strive for a shrewd and sympathetic delineation of character; they tend to describe political or diplomatic crises; and their work sometimes exhibits the dramatic tension typical of a novel. (See Garrett Mattingly, *The Armada*, New York, Houghton Mifflin, 1959; Dom David Knowles, *The Historian and Character*, Cambridge, Cambridge University Press, 1955.)

(2) *Biography.* Historians sometimes approach the history of a period through writing the story of the life of some great or representative man. The editor of a series of historical works based on this principle says:

> "I am convinced that the most congenial, as well as the most concrete and practical, approach to history is the biographical, through the lives of the great men whose actions have been so much part of history, and whose careers in turn have been so moulded and formed by events. The key idea of this series. . .is the intention by way of a biography of a great man to open up a significant historical theme: for example, Cromwell and the Puritan Revolution, or Lenin and the Russian Revolution." (A.L. Rowse, *The Use of History*, London, Hodder and Stoughton, 1946, p. vi.)

(3) *Prosopography.* This is the technique of investigating political history by means of multiple biographies of many important and less important political figures. It was applied to Roman history by Sir Ronald Syme and to the history of eighteenth-century England by Sir Lewis Namier. In Namier's *The Structure of Politics at the Accession of George III* (2 vols., London, Macmillan, 1929), biographical sketches of the members of mid-eighteenth-century parliaments were used to show the importance of family ties or "connections" and the unimportance of the traditional party affiliations of Whig and Tory in the parliamentary groupings of that time. The technique of multiple biography has subsequently been applied to the members of religious groups and to the parliamentary history of other periods. (See also Lawrence Stone, "Prosopography," *Daedalus*, Winter, 1971.)

(4) *Psychohistory.* William Langer, in his 1957 presidential address to the American Historical Association, urged historians to enrich their narratives by using the findings and, where possible, the techniques of depth psychology. ("The Next Assignment," *American Historical Review*, Vol. 63, 1958.) The leading exponent of such an approach is the psychoanalyst Erik H. Erikson, but increasing numbers of historians have taken training in psychology or psychoanalysis and have begun to publish psychoanalytically oriented biographies. Erikson's books *Young Man Luther: A Study in Psychoanalysis and History* (New York, Norton, 1958) and *Gandhi's Truth* (New York, Norton, 1969) are among the most influential in this area. Other works, by historians, are those of William B. Willcox, *Portrait of a General: Sir Henry Clinton in the War of Independence* (New York, Knopf, 1964) and Rudolf Binion, *Frau Lou: Nietzsche's Wayward Disciple* (Princeton, Princeton University Press, 1968). For a brief recent psychoanalytic study, see Otto Pflanze's "Toward a Psychoanalytic Interpretation of Bismarck" (*American Historical Review*, Vol. 77, 1972).

Psychohistory need not be limited to biography; Langer's original address emphasized mass responses to catastrophes such as the Black Death of the fourteenth century. (For further examples, see Johann Huizinga, *Waning of the Middle Ages*, New York, Doubleday, 1954, and Robert Jay Lifton, *Death in Life: The Survivors of Hiroshima*, New York, Random House, 1968.)

Emphases on Physical and Economic Influences

(1) *Deterministic Views of History.* Influenced by nineteenth-century philosophical materialism, Henry Thomas Buckle argued that all historical development was ultimately determined by climate and other geographical influences. (*History of Civilisation in England*, 2 vols., London, J.W. Parker, 1857-61.) A similar position was taken by Ellsworth Huntington, in works such as *Mainsprings of Civilization* (New York, Wiley, 1945).

Others have maintained that technological innovation is the only or the decisive determinant of historical change. That viewpoint is of considerable antiquity; it was a commonplace of the late Renaissance that the inventions of printing and gunpowder had decisively changed European society (although both had been known for centuries in China without having had similar effects). The stirrup and the windmill have also been described as technological changes having great impact on society (see Lynn T. White, Jr., *Medieval Technology and Social Change*, Oxford, Oxford University Press, 1962).

Some historians have maintained that immediate economic interests are either preeminent or strongly influential in determining historically important decisions. Charles Beard emphasized the role of the financial interests of those who drew up the U.S. Constitution, particularly their holdings in government securities, on the form that document took. (*An Economic Interpretation of the Constitution of the United States*, New York, Macmillan, 1913.) However, he later warned against "oversimplified economic interpretations" that view economic interests as if "they are always and everywhere independent 'causes' of political actions and institutions." (*The Economic Basis of Politics*, 3rd rev. ed., New York, Knopf, 1945.)

The Marxist account of "historical materialism," although emphasizing economic factors, is not a simple economic determinism. Marxists argue that the economic base of a society is the primary, but not the only, determinant of social change. The economic base consists of the "forces of production" (the raw materials and natural resources available to a society and its tools and technology) and the "relationships of production" (the social and economic structure in which goods are owned and exchanged). The forces of production may be similar in industrialized economies, but the relations of production may be markedly different in socialist and capitalist economies. The economic base primarily determines the configuration of a society's "superstructure" (its laws, political institutions, art, religion, philosophy, and science), but the superstructure may also influence the base.

(2) *Reliance on Econometric and Demographic Methods.* Although economic history is an old field, recently econometric analysis has been applied to historical materials. The field is sometimes called "cliometrics." For example, Alfred H. Conrad and John R. Meyer attempted to discover the rate of return on investment in Negro slaves, and on the basis of their findings concluded that slavery in the antebellum South was not unprofitable, as claimed by most previous historians. ("The Economics of Slavery in the Ante-Bellum South," *Journal of Political Economy*, Vol. 66, 1958.) Another influential work in "retrospective econometrics" is Robert W. Fogel's *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore, Johns Hopkins University Press, 1964). After calculating the costs of alternative forms of transport and storage, Fogel reached the conclusion that if railroads had not been invented, the gross national product of the United States in 1890 would have been almost as high as the figure actually attained. (See also Ralph L. Andreano, ed., *The New Economic History*, New York, 1970, and Thomas C. Cochran, "Economic History, Old and New," *American Historical Review*, Vol. LXXIV, 1969.)

Another quantitative approach is that of historical demography. Because there were local censuses and systems of registrations of baptisms, marriages, and burials in Europe as early as the sixteenth century (and far earlier in China), the history of some populations can be studied for periods before the beginnings of regular national censuses in the eighteenth century. Demographic methods provide a quantitative way of measuring aspects of the "welfare" of past societies (through mortality figures and life expectancies) and for noting changes in moral sanctions (rates of bastardy and pre-nuptial pregnancy and the spread of family limitation); they can also help to provide basic background materials for a general social history in which the relationships of wages, prices, housing, and occupational distribution can be described. French historians have been especially active in this field; notably Pierre Goubert (*Beauvais et le Beauvaisis de 1600 à 1730*, Paris, S.E.V.P.E.N., 1960) and Emmanuel Le Roy Ladurie (*Les paysans de Languedoc*, Paris, S.E.V.P.E.N., 1966). Introductions to this field in English are the *Daedalus* issue entitled "Historical Population Studies" (Spring, 1968), and E.A. Wrigley, *Population and History* (London, Weidenfeld and Nicolson, 1969).

For a general consideration of quantitative methods, see William O. Aydelotte, Allan Bogue, and Robert W. Fogel, eds., *The Dimensions of Quantitative Research in History* (Princeton, Princeton University Press, 1972).

Universal Process Histories

Most claims to reveal "the meaning of history as a whole" emphasize history as a universal process tending toward some goal. This may be the Last Judgment and final triumph of righteousness, as in Christian theological histories, or the achievement of some utopia on earth, as envisioned by such enthusiastic advocates of the idea of progress as Condorcet. Many of the "scientific" historians of the late nineteenth and early twentieth centuries wrote their monographs in the belief that history as a whole was the story of the steady progress of humanity toward higher living standards or the establishment of representative government and individual freedom. (See, for example, Herbert Butterfield's account of such views in *The Whig Interpretation of History*, London, Bell and Sons, 1931.)

A different notion of progress may be found in the writings of Hegel and his followers. For Hegel, the entire historical process was the progressive self-realization of Spirit, which could only come to self-knowledge through action. Among the Left Hegelians, Karl Marx attempted to eliminate all traces of that kind of Idealism from his account of history, but he retained the notion of cataclysmic discontinuities between epochs (brought about by revolutions rather than by Hegel's "world-historical individuals") and saw historical processes as inevitably producing the triumph of the proletariat, the elimination of private property, and the cessation of class warfare.

Although most "universal process" historical accounts are teleological, they need not be. One example is Rushton Coulborn's work, *The Origin of Civilized Societies* (Princeton, Princeton University Press, 1959). Toynbee's work also may be mentioned.

4. RESULTS ACHIEVED

Our discussion of results achieved in historical inquiry overlaps the discussion of current controversies in the next Section. Many historical accounts achieve a high level of literary merit, and the imaginative reconstructions of past events often seem to have a striking plausibility for many readers. For purposes of this book, the important question is the extent to which historical inquiry yields warranted assertions.

Some historians have taken a skeptical attitude toward the possibility and even the desirability of developing accurate descriptions of past events. Carl Becker, for example, regards historical reconstructions as social myths that are important guides to action. He says:

> "The history that does work in the world, the history that influences the course of history, is living history, that pattern of remembered events, whether true or false, that enlarges and enriches the collective specious present. . . . Every generation, our own included. . .must inevitably play on the dead whatever tricks it finds necessary for its own peace of mind. The appropriate trick for any age is not a malicious invention designed to take anyone in, but an unconscious and necessary effort on the part of 'society' to understand what it is doing in the light of what it has done and what it hopes to do. We, historians by profession, share in this necessary effort. . . . Our proper function is not to repeat the past, but to make use of it, to correct and rationalize for common use Mr. Everyman's mythological adaptation of what actually happened." (Becker, *op. cit.*, p. 235, pp. 252-253.)

Others have argued that historians do arrive at accurate descriptions of what happened. Jack Hexter, for example, says:

> "Truth about history is not only attainable but is regularly attained. It is true, for example, that at Waterloo on 18 June 1815 Napoleon I and his army were decisively defeated by a coalition army commanded by the Duke of Wellington. This is true in the simple sense that it is an *accurate* description of something that happened in the past, and the accurate description of things that happened in the past is one of the ends of history writing." (*Reappraisals in History*, Evanston, Northwestern University Press, 1961, p. 189.)

Accurate descriptions of the type mentioned by Hexter do seem to be "regularly attained" by historians. The difficulties and controversies arise when more general interpretations are involved about the developmental sequences leading to some complex event. Various interpretations are assessed differently by different historians, and sometimes later interpretations, presumably based on the cumulative weight of the evidence, are viewed as inferior to earlier interpretations. Page Smith, for example, disparages recent interpretations of the American Revolution:

> "[T]he best interpretation of the causes of the Revolution was made in the decade following the treaty of peace in 1783 and. . .thereafter, as we moved further in time from the dramatic events of the Revolution and brought to bear on the problem all the vast resources of modern scholarship, we moved further and further from the truth about our Revolutionary beginnings." (*The Historian and History*, New York, Vintage Books, Random House, 1960, pp. 165-166.)

And Howard K. Beale has argued that despite all the benefits of hindsight, historians working on the American Civil War have not developed any "explanations" that were not "comprehended and stated before the war occurred." (1946 *Report* of the Committee on Historiography, p. 88.)

Some progress apparently often is made in terms of rejecting untenable conjectures or hypotheses. To illustrate, after a consideration of the historical accounts of the nineteenth-century Prussian statesman, Baron von Stein, Klaus Epstein says: "No serious scholar believes today that Stein had an unproblematical personality (as asserted by Pertz), or that he drew his main inspiration from the 'ideas of 1789' (as asserted by Lehmann), or that all was well with Prussia in 1806 (as asserted by Meier)." ("Stein in German Historiography," *History and Theory*, Vol. 5, 1966, p. 274.)

Such discussions are practically certain to lead to continual disagreement and controversy as long as historians have so many different aims; the criteria for the acceptance of interpretations varies from what is regarded as intrinsically plausible to those associated with the development of warranted assertions as discussed in Chapter I. Giving up the quest for absolute truth need not result in complete skepticism, although sometimes historians write as if those were the only alternatives.

The difficulties just discussed are compounded because historical descriptions typically rely upon generalizations from other fields. Sometimes those generalizations are so well supported by the evidence that they lead to warranted assertions, as in the use of X-ray photography and carbon-14 analysis in the verification of documents. Other physical science techniques and findings also can be useful to historians. For example, the extent and distribution of cereals cultivation, even of a millennium ago, can be traced by means of the chemical analysis of pollen preserved in peat bogs. Even the physical appearance of Iron Age men is revealed in the preservation of their bodies in peat. (See P.V. Glob, *The Bog People: Iron-Age Man Preserved*, translated by R.L. Bruce-Mitford, London, Faber, 1969.)

Much progress has been made in the analysis of written documents, which now involves not only philological techniques but physical science techniques. The exposure of the forged "Donations of Constantine" by Lorenzo Valla in the fifteenth century, based on anachronisms and inconsistencies in the document, was one of the early impressive achievements using philological techniques, and was followed by much work along similar lines. In general it seems safe to say that numerous useful results have been achieved by historians in the authentication and dating of documents.

However, the generalizations used by historians from some other fields may be far more dubious, as in the reliance on psychoanalytic or other scientifically unverified speculations. Robert Fogel, for example, says that: "The models of theoretical economics represent a library–very much like the library of computing routines at an IBM installation–which can be drawn on in an analysis of a wide range of historical problems." (Fogel, *op. cit.*, p. 246.) But inasmuch as many economic models have proven not to yield useful predictions, their use in historical inquiry can be seriously misleading.

In general, then, we can say that historians typically make use of generalizations in their attempts to ascertain what happened in the past. The results can range from the scientific to the highly speculative, depending upon the generalizations used and the objective of the historian.

5. CONTEMPORARY CONTROVERSY

For some time there has been considerable discussion among historians, philosophers, and behavioral scientists about methods of historical inquiry, with emphasis on such questions as the extent to which historical inquiry can be scientific and whether or not it is desirable for historians to be scientific. Many historians are not as concerned with methodological questions as are workers in other behavioral science areas. Higham expresses the views of some historians when he says:

> "Connections between theory and practice in historical work are usually circuitous and indistinct. The fluid, unsystematic character of the historian's enterprise rarely permits him to go directly from a general theory to a particular proof. He is even likely to be a bit unclear about what his historiographical assumptions are. . . . In fact, a taste for theoretical niceties, a strong urge for clarity and precision in basic assumptions, can actually prove a handicap in dealing effectively with historical data. To move freely through the complex web of human experience, historians need to employ simultaneously a multitude of causal hypotheses. Accordingly, a good historian is not likely to operate consistently within a single theoretical framework; any one perspective restricts his range of vision. Like literature, history can gain richness from the interpenetration of conflicting ideas, from the tensions of a divided allegiance." (Higham, *op. cit.*, p. 147.)

The 1946 *Report* of the Committee on Historiography describes a situation that still seems characteristic of the field of history:

> "The profession, even in its outstanding leaders, can hardly be said even as yet to have achieved complete clarity as to its methodological principles. And large numbers, who greatly admire in others the fruits of the practice of a functional history, and perhaps ably carry it on themselves, nevertheless in their own theory of history retain many elements from the assumptions of earlier days. But in this lag the historical profession is scarcely unique. It is notorious that most natural scientists are apt to be none too clear when they try to analyze all that is implied in the very methods they themselves may be so brilliantly exemplifying. In stating their methods, they are very likely to fall back on what they early learned their methods should be, oblivious of how they themselves have improved on what they were taught." (p. 51.)

Possibly it is for such reasons that philosophers rather than historians have frequently formulated many of the positions involved in controversies about historical inquiry. Much of the discussion concerns the issue of whether history is a field unlike all others (*sui generis*), is a part of behavioral science, or is a literary endeavor. Arthur Danto argued that historical inquiry is not, could not, and should not be scientific, and that "it is a special and irreducible activity of the human spirit, with a function and justification of its own." ("On Explanations in History," *Philosophy of Science*, Vol. 23, 1956, p. 15.)

Robert Stover, in a book that he hoped would put an end to the "sterile controversy" between those who regard history as a science and those who see it as a discipline *sui generis*, argues against opting for either alternative exclusively. In his view, historical thinking can be carried on both from a natural science (deterministic) viewpoint and from a personally and socially "caring" viewpoint. The first aims at intelligibility and the second at living and acting in the world. Both are found together; they are not mutually exclusive. (*The Nature of Historical Thinking*, Chapel Hill, University of North Carolina Press, 1967.)

Along somewhat similar lines, Patrick Gardiner says:

> "At one extreme lies the view that history is a branch of knowledge which is *sui generis*; at the other, there is the claim that it is, in some sense, a department of science or, at any rate, that it is capable of being transformed into such a department. . . . I shall suggest that there is truth in both the contending doctrines." (*The Nature of Historical Explanation*, Oxford, Clarendon Press, 1952, p. 32.)

Both the issues just mentioned and others are related to disagreements concerning the relation of the unique, the particular, and the individual to generalizations in historical inquiry. Although some historians have maintained that historians investigate the unique, whereas the sciences are concerned with what is uniform in recurrent events, others have rejected that viewpoint. Edward H. Carr, for example, says: "The historian is not really interested in the unique, but in what is general in the unique." (*What Is History*?, New York, Knopf, 1962, p. 83.) And Geoffrey Elton argues that no one can "deal in unique fact, because facts and events require reference to common experience"; he insists, however, that historians focus on "individual and particular" facts and events that are "treated as peculiar to themselves." (Elton, *op. cit.*, p. 11.)

Elton also rebukes philosophers for "hindering the practice of history" and rejects the notion that history is one of the "so-called social sciences." He regards as an advantage that on his view there can be no experimental verification of historical conjectures. Scientists manipulate their subject matters and can only study "specifically prepared artificial derivatives from what naturally occurs," whereas historians inquire into "a dead reality independent of the enquiry." This seems to overlook not only physical "manipulations" of historical subject matter (e.g., carbon-14 dating), but also the transactional relations of any inquirer and his subject matter.

Much debate has occurred in relation to Carl G. Hempel's view that historical explanation requires general laws. ("The Function of General Laws in History," *Journal of Philosophy*, Vol. 39, 1942.) Hempel rejected the old nomothetic-idiographic distinction between sciences that yield general laws and those that yield knowledge of unique or individual events. He believes that an explanation of an event requires a set of causal conditions and at least one "universal hypothesis," and that explanation in all the empirical sciences has the same general form. Just as does the natural scientist, the historian:

> "...aims at showing that the event in question was not 'a matter of chance,' but was to be expected in view of certain antecedent or simultaneous conditions. The expectation referred to is not prophecy or divination, but rational scientific anticipation that rests on the assumption of general laws." (*Ibid.*, p. 39.)

Hempel admits that historians typically do not explicitly invoke general laws, which in his view is required for an explanation, and says that instead they provide "explanation sketches":

> "Such a sketch consists of a more or less vague indication of the laws and initial conditions considered as relevant, and it needs 'filling out' in order to turn into a full-fledged explanation. This filling-out requires further empirical research, for which the sketch suggests the direction." (*Ibid.*, p. 42.)

Hempel sees, then, two general possibilities open to historians:

> "The interpretations which are actually offered in history consist either in subsuming the phenomena in question under a scientific explanation or explanation sketch; or in an attempt to subsume them under some general idea which is not amenable to any empirical tests.... [The latter is only] a pseudo-explanation which may have emotive appeal and evoke vivid pictorial associations, but which does not further our theoretical understanding of the phenomena under consideration." (*Ibid.*, p. 45.)

Subsequently Hempel modified his views somewhat. He agrees that historians only infrequently use general laws of the "deductive-nomological" type that physical scientists often use and that permit the deduction of an event from the law. Rather, historians typically rely on "statistical-inductive" laws, which are statements of the probability of some event's occurring.

One of Hempel's chief critics is William Dray. Dray supports *verstehen* procedures of inquiry. Humanistic studies, he holds, strive for an appreciation of the agent's reason for acting, whereas scientific investigations describe behavior in terms of regularities; "history is logically continuous with literature rather than social science." (*Laws and Explanation in History*, London, Oxford University Press, 1957, p. 139.) Human action must be understood from "the actor's point of view," according to Dray: "only by putting yourself in the agent's position can you *understand* why he did what he did." History is characterized by a special logical type of explanation that Dray calls "rational explanation," in which the aim is "to show that what was done was the thing to have done for the reasons given." Although many human behaviors may be lawful, "discovery of the law would still not enable us to understand them in the sense proper to this special subject-matter." (*Ibid.*, pp. 118-128.)

At present, many writers on historical methodology adopt procedures similar to those of Dray and regard *the narrative* as a, or the, distinctive form of historical understanding. For example, Arthur Danto discusses in detail the thesis that "narrative sentences are so peculiarly related to our concept of history that analysis of them must indicate what some of the main features of that concept are." (*Analytical Philosophy of History*, Cambridge, Cambridge University Press, 1965, pp. 143-151.) And W.B. Gallie contends that stories are as distinctive to history as theories are to the natural sciences: "from the moment an historian first conceives his task, he conceives it as a certain kind of story, with a roughly described track of development toward a main conclusion." (*Philosophy and the Historical Understanding*, London, Chatto and Windus, 1964, pp. 71-72.) Gallie compares understanding history to following a story or watching a game of cricket, where explanations are only called for when some unexpected turn has occurred, and where understanding is guided by the anticipated end.

Recently Lee Benson has been highly critical of literary or narrative history and has called for a scientific method of historical inquiry. He says:

> "In my judgment, history has become a relatively trivial enterprise. I cannot think of any significant contribution that specialists of past human behavior have made to social thought in the last fifty years.... I think it is reasonable to claim that no historian writing in the last fifty years has significantly contributed to our thinking about the nature of human beings, the way human beings function, or how the study of human behavior can help men achieve desired social goals." (In Samuel H. Beer *et. al.*, "New Trends in History," *Daedalus*, Fall, 1969, p. 891.)

Benson also maintains that historical inquiry "attempts to understand the past in order to control the present and future." (*Ibid.*, p. 892.) From that point of view, he criticizes historical scholarship about the American Civil War, which probably has had more man-years of historical effort devoted to it than has any other phenomenon. The results, according to Benson, are trivial: "If one were called before some bar to justify the incredible amount of resources and intelligence and effort expended in this field, truthfulness would force one to be silent." (*Ibid.*, p. 901.) He maintains that the information resulting from all this work has no relevance to other nations in which different subcultures are geographically separated, and does not "specify the conditions under which political systems are likely to be unable to resolve differences through the normal peaceful means that the system provides." (*Ibid.*, p. 902.)

There are numerous other controversies among historians about methodology, only one of which will be mentioned here. Although historians of many shades of political opinion have sometimes questioned the possibility of ethical neutrality in historical inquiry, recently that point of view has been strongly emphasized by some historians favoring radical social change. Howard Zinn, for example, says that in "a world where justice is maldistributed, historically and now, there is no such thing as a 'neutral' or 'representative' recapitulation of the facts." He also calls for "an emphasis on those historical facts which have hitherto been obscured, or whose recall would serve to enhance justice and brotherhood." (*The Politics of History*, Boston, Beacon Press, 1970, p. 24.)

As in many other fields, some historians have bitterly attacked the links they see between most of the profession and the socioeconomic establishment. Barton J. Bernstein's *Towards a New Past: Dissenting Essays in American History* (New York, Pantheon, 1968) contains representative articles by radicals, left-liberals, Marxists, and other critics.

6. TERMINOLOGICAL PROBLEMS

Most historians have almost exclusively used ordinary literary language in their inquiries, which has resulted in continual difficulties with ambiguities in terminology. These difficulties were examined in the 1946 *Report* of the Committee on Historiography. The Committee selected fifty terms frequently used in historical writing and collected numerous examples of their use. They commented: "it may be said that the results were fearful and wonderful."(*Op. cit.*, p. 107.) To see if such confusion could be reduced, the Committee asked Sidney Hook, a philosopher well acquainted with historical method, to study the fifty terms and "formulate definitions as exact as possible" on the basis of the material submitted to him. After many conferences with the Committee, Hook "made it evident to the members that no self-consistent definitions of the fifty historical terms could be based on an analysis and synthesis of the meanings given to them by numerous historians." Among the terms Hook analyzed were: *accident, analogy, cause, chance, change, contingency, destiny, development, dialectic, fact, fate, force, frame of reference, generalization, predestination, progress*, and *understanding.*

Since that time an effort has been made by an international team of historians to clarify terminology (E. Lousse, ed., *Grundbegriffe der Geschichte*, Berlin, de Gruyter, 1965), and another group of historians has studied "generalization" (Louis R. Gottschalk, ed., *Generalization in the Writing of History*, Chicago, University of Chicago Press, 1963). Neither attempt has enjoyed much success, except in demonstrating the necessity of further work on the difficulties involved.

"Cause" and related names have been much discussed. Some historians suggest that the name should be abandoned. (See Charles Beard and Alfred Vagts, in the 1946 *Report* of the Committee on Historiography, pp. 136-137.) Patrick Gardiner, on the other hand, argues that historians' language would have to be entirely recast if all references to "cause" and related language were eliminated. (Gardiner, *op. cit.*, pp. 65 ff.) Isaiah Berlin has written an influential criticism of deterministic views of history. Although he does not maintain that determinism is "necessarily false," he says that we can "neither speak nor think as if it could be true" and that if determinism were correct many distinctions commonly made "would be as inappropriate as the attribution of moral responsibility to the planetary system or the tissues of a living cell." (*Historical Inevitability*, London, Oxford University Press, 1954, pp. 32-33.)

As already noted, many historians put special emphasis on "explanation." Sometimes "explanation" names an adequate description of things and events. At other times, it names a special form of understanding of motives and intentions. Despite the confidence of many writers in history and elsewhere that motives and intentions are the way to explain human actions, often the alleged explanation is only a restatement of the question with which we began the inquiry, as was discussed in Chapter 1. If we want an answer to the question of why Hitler behaved as he did on certain occasions (in the sense of wanting to describe adequately the connections among the things and events leading up to his behavior), to be told that he was aggressive, wanted to enlarge Germany's geographic area, etc., is simply to restate the original question.

7. COMMENT AND EVALUATION

Practicing historians tend to blend literary and behavioral science approaches, and sometimes that is viewed as a great virtue. Higham, for example, praises the historian as uniquely:

> ". . .viewing a situation from within and from above, blending subjective identification with objective analysis, uniting art with science, recognizing the complementarity of perspectives and the multiplicity of relationships by which the historian—and he alone—undertakes to grasp a transition in human affairs in its full contextual significance." (Higham, *op. cit.*, pp. 143-144.)

Even when historians emphasize behavioral science procedures of inquiry, they often use the less scientific aspects of those procedures. Robert F. Berkhofer, Jr., for example, although advocating a behavioral approach, defends subjectivist procedures such as phenomenology and *verstehen.* (*A Behavioral Approach to Historical Analysis*, New York, Free Press, 1969.)

If we turn to the question, not of what historians actually do, but of what they could do, there seems to be no insurmountable methodological difficulty in approaching historical inquiry in the way suggested in Chapter 1. And, indeed, the results of historical inquiry that best stand up seem to be those in which such a method was used, whether or not the historians were fully aware, or aware at all, of that method.

The major methodological controversies concerning historical inquiry are also found in other behavioral science areas. Historians usually discuss those controversies in a cruder way than is found in the other fields, and are often convinced that their field is somehow "special" and must therefore have a special general method. However, inquirers in other areas often deal successfully with historical materials, without using "special" methods.

Frequently the focus on particular events is what is taken as requiring some special method. However, physical scientists are often concerned with specific happenings at specific times and places, either in the future, as in predicting the next eclipse of the moon, or in the past, as when geologists date some phenomenon. Moreover, typically in experimental situations the scientist needs to have an accurate description of just what happened to one variable as a consequence of a change in another variable. Sometimes historians maintain that their interest in particular events is just in those events themselves, rather than in the events as representative of a class of similar events. But that distinction seems to be one only of degree, not kind; the scientist's interest in an eclipse may be as "particular" as the historian's interest in the events in his subject matter, and the historian uses generalizations in describing particular events, just as the physical scientist does. Perhaps of even greater importance, human transactions are such that separating, detaching, or severing the individual organism from its environment typically impedes inquiry. Emphasizing individual aspects as individual, rather than viewing them in the full process of which they are a part, often makes it more difficult to develop useful descriptions of the connections among the aspects and phases of the transaction being investigated.

Lee Benson is one of the few historians urging that something like what we called modern scientific procedures of inquiry be used to replace the "established historiographic system" (*Toward the Scientific Study of History*, Philadelphia, Lippincott, 1972, p. 227). His negative criticism of conventional Civil War historiographers—that like the Buddhist wheel of fate they go "round and round" without ever getting anywhere—seems well founded. He has demonstrated the unreliability and inadequacies of much of the data used by historians. He emphasizes that behavioral inquiry should not be modeled after, or restricted to, the specific investigatory techniques used by physical scientists.

In general, much of Benson's work is in harmony with the views we expressed in Chapter 1. On the other hand, he disagrees in important ways with some of the points we emphasize. He apparently does not accept our view about the need to interweave conjectures and measurements at *all* stages of inquiry, for he develops in advance of testing an elaborate "general analytical model for explanations of internal war causation" (*Ibid.*, pp. 334-340), and in other ways appears to defend elaborate *a priori* conjecturing that we regard as inappropriate. His break with the literary model for historical inquiry, however, may set a desirable precedent for the profession.

Whether what is done by professional historians will largely shift to the type of inquiry we called "modern" in Chapter 1, we do not attempt to predict, but we do suggest that there are no intrinsic difficulties in the subject matter that would preclude such inquiry, that the results of such inquiry have proved useful, and that we see no reason why the results will not continue to prove useful in the future.

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9. GERMANE JOURNALS

AMERICAN HISTORICAL REVIEW
AMERICAN SLAVIC AND EAST EUROPEAN REVIEW
ANNALES: ECONOMIES, SOCIETIES, CIVILISATIONS
CATHOLIC HISTORICAL REVIEW
COMPARATIVE STUDIES IN SOCIETY AND HISTORY
ECONOMIC HISTORY REVIEW
ENGLISH HISTORICAL REVIEW
HISPANIC AMERICAN HISTORICAL REVIEW
HISTORICAL ABSTRACTS
HISTORICAL JOURNAL
HISTORISCHE ZEITSCHRIFT
HISTORY AND THEORY
JOURNAL OF AMERICAN HISTORY
JOURNAL OF CONTEMPORARY HISTORY
JOURNAL OF ECONOMIC HISTORY
JOURNAL OF MODERN HISTORY
JOURNAL OF NEGRO HISTORY
JOURNAL OF THE HISTORY OF IDEAS
MIDDLE EAST JOURNAL
PACIFIC HISTORICAL REVIEW
PAST AND PRESENT
RIVISTA STORICA ITALIANA
REVUE HISTORIQUE
SAECULUM
SPECULUM
VOPROSY ISTORII

VIII.

JURISPRUDENCE*

1. WORKING DESCRIPTION OF THE FIELD

INQUIRERS in the field of jurisprudence study the origin, development, and operation of legal institutions, rules, and processes, including their connections with other sociocultural processes and institutions. The field overlaps many other behavioral areas of inquiry, including sociology, political science, economics, psychology, and anthropology.

2. OTHER DESCRIPTIONS OF THE FIELD

The label "jurisprudence" has been applied in various ways; e.g., to designate a particular legal system (as in "Roman jurisprudence"); the course of judicial decisions over time; a point of view about legal behavior (as in "sociological jurisprudence"); inquiry into assumed underlying principles of legal systems ("philosophy of law" and "formal science of law"); and law as a subject matter of inquiry. Investigators in the field of jurisprudence often are labeled *jurists* or *jurisprudents*.

Historically jurisprudence has been associated with inquiries in many other fields, including philosophy and theology. For some time, many writers have urged that jurisprudence become a behavioral science, fully or partially. Roscoe Pound, one of the most influential recent jurists, says:

> "The thirteenth century put theological philosophy behind law to sustain authority. The sixteenth and seventeenth centuries divorced philosophy of law from theology and divorced law (not laws) from authority. The nineteenth century divorced legal philosophy from political philosophy and set jurisprudence off definitely as a separate science. The twentieth century seeks to unite jurisprudence with the other social sciences. . . " (*Law Finding Through Experience and Reason*, Athens, University of Georgia Press, 1960, p. 17.)

On the other hand, many jurists maintain that jurisprudence fundamentally is "normative" rather than scientific. For example, in a discussion of world order, D.P. O'Connell says:

> "A jurist who discusses 'order' has in mind a particular and fairly restricted concept of order. Unlike the natural or social scientists, he is not concerned with problems of hypothesis and verification, or limited to describing events and analyzing tendencies. His is a discipline concerned with the control of human behavior, and it is therefore normative. In a colloquium on 'world order' it is therefore important that the jurist insist that the normative element be retained in the discussion, for, if he fails to do so, he abandons his claim to keep his study discrete from politics. He may well consider that the activities of the political scientists or economists or experts on international relations are properly concerned with no more than the ascertainment of trends and the prediction of consequences. His own discipline requires something more, and that is the element of value." ("The Role of International Law," *Daedalus*, Spring, 1966, p. 627.)

Some writers who view jurisprudence as a social science field also believe that the social sciences, unlike the physical sciences, are concerned with "normative" issues. Pound, for example, says: "What-ought-to-be has no place in physical science. It has first place in the social sciences." (*Contemporary Juristic Theory*, Claremont, Claremont College, 1940, p. 36.)

As in the fields of economics and political science, there are many diverse views found about the relation between "positive science" and "policy applications." Eugen Ehrlich, a German sociological jurist who had considerable influence on many American jurists, emphasized the "theoretical" and "pure knowledge" in jurisprudence:

> "In jurisprudence. . .the distinction between the theoretical science of law. . .and the practical science of law. . .is being made only just now. . . . This distinction. . .is the basis of an independent science of law, whose purpose is not to subserve practical ends but to serve pure knowledge, which is concerned not with words but with facts." (*Fundamental Principles of the Sociology of Law*, Cambridge, Harvard University Press, 1936, pp. 3-4.)

Others viewed jurisprudence as "practical" and as concerned with improving the operation of legal processes. Sometimes jurisprudence is viewed as "social engineering":

> "Jurisprudence. . .is 'social engineering' and the various trends within it. . .are only different techniques of such engineering suited to the interpretation of particular needs of concrete systems of law and corresponding types of inclusive societies." (Georges Gurvitch, *Sociology of Law*, New York, Philosophical Library, 1942, p. 11.)

And Pound said:

> "Engineering is thought of as a process, as an activity, not merely as a body of knowledge or as a fixed order of construction. . . . The engineer is judged by what he does. His work is judged by its adequacy to the purposes for which it is done. . . . We are beginning, in contrast with the last century, to think of jurist and judge and lawmaker in the same way." (*Interpretations of Legal History*, New York, Macmillan, 1923, p. 152.)

Some writers emphasize problem-solving:

> "The important thing is that scientific jurisprudence is essentially a problem-solving device. . . . Experimental jurisprudence may be said to extend

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*Samuel Krislov suggested changes in an earlier version of this chapter. His comments are appreciated, but he is not responsible for the content of the chapter.

to two main fields of activities: (1) bringing the discoveries of other sciences, both physical and social, into such a focus that they can be used as tools to aid in lawmaking and enforcement; (2) conducting legal research into the effectiveness of statutes as actually enforced to accomplish the purpose for which they were enacted, and. . .to develop by research and experiment the jural laws controlling such phenomena." (Frederick Beutel, *Some Potentialities of Experimental Jurisprudence as a New Branch of Social Science*, Lincoln, University of Nebraska Press, 1957, pp. 19, 189.)

3. METHODS AND TYPES OF INQUIRY

Pound identifies over 20 separate schools of jurisprudence, dating from the Greeks and the Romans (*Jurisprudence*, St. Paul, West Publishing Co., 1959), and others could be added. For present purposes we shall discuss briefly some of the traditional schools and give greater emphasis to those points of view that are intended to be scientific or partly scientific.

Natural Law Jurisprudence. Natural law "theory" is derived from Greek, Roman, and Christian sources, and has been strongly influential in American political and legal history. In the Thomistic version, four kinds of law are differentiated: (1) "eternal law," the reason of God that governs the universe; (2) "divine law," the revelation of divine wisdom in the Bible; (3) "natural law," the reflection of divine reason in created things and inherent in human nature; and (4) "positive" or "human law," the laws promulgated by governments. Natural law is said to be stable and universal [illegible] to apply to all cultures and all men, including pagans. Thomas Aquinas mentioned as examples of natural law [illegible] inclination of men to live in society, to preserve themselves, to beget and raise children, etc. Positive laws, according to the Thomists, vary from place to place and must be adapted to particular circumstances, but natural laws are unchanging *a priori* standards for judging positive laws.

The theological basis of Thomistic natural law is defended by Jacques Maritain:

> "This true philosophy of the rights of the human person is based upon the true idea of natural law, as looked upon in an ontological perspective and as conveying through the essential structures and requirements of created nature the wisdom of the Author of Being." (*Man and the State*, Chicago, University of Chicago Press, 1951, p. 84.)

Many other versions of natural law, including secular versions, have been offered. The "source of knowledge" of natural law sometimes has been been held to be revelation, sometimes "pure" reason, and sometimes revelation plus reason. Recently some writers have suggested that the biological and the behavioral sciences might provide an empirical basis for natural law. See, for example, F.S.C. Northrop, *The Complexity of Legal and Ethical Experience* (Boston, Little, Brown, 1959). And many writers who reject earlier, overtly theological, versions of natural law maintain that in some sense natural law exists. Philip Selznick, for example, argues that "reason" can select those human norms that properly are to be given the sanction of law. ("The Sociology of Law," *Journal of Legal Education*, Vol. 12, 1960, pp. 521-522.) Lon L. Fuller perhaps has been the most influential recent jurist defending natural law; see his *The Law in Quest of Itself* (Chicago, The Foundation Press, 1940) and his *Anatomy of the Law* (New York, Praeger, 1968). For some general criticisms of natural law doctrines, especially in relation to the United States, see Cornelia G. Le Boutillier, *American Democracy and Natural Law* (New York, Columbia University Press, 1950); and Eugene C. Gerhart, *American Liberty and "Natural Law"* (Boston, Beacon Press, 1953).

Analytic Jurisprudence. The older school of analytic jurisprudence arose in part because of the need for organization in the common law of England. John Austin dealt systematically and critically with the law of his time. (*Lectures on Jurisprudence*, 5th ed., revised, London, John Murray, 1885.) The law was analyzed into taxonomic divisions (e.g., contracts, property, etc.), in order to show the logical interrelations of the various fields. Positive law was viewed as the subject matter of jurisprudence, and law was identified with the "command" of a "sovereign power." Austin gave little or no attention to the historical and sociological processes through which laws are developed; he analyzed logically the rules and materials found in the legal system under investigation. This procedure, somewhat modified, still has followers in Great Britain (G.W. Paton, *A Text-book of Jurisprudence*, Oxford, Clarendon Press, 1946; and earlier, T.E. Holland and J.W. Salmond).

In addition to the Austinian school, other points of view also emphasize some form of "analytic" procedure. For example, Hans Kelsen attempted to discover the inherent structure of legal systems, and argued for a "Pure Theory of Law" that would exclude everything of a nonlegal character. He held that the law was a self-contained system of propositions consisting of declarations by officials. Kelsen made a sharp distinction between social facts and the logical consequences of a legal system's postulates. The "validity" of positive law depends upon a "basic norm." The basic norm is that norm the validity of which cannot be derived from a higher one. Basic norms seem to be the presuppositions that allegedly make possible the rest of the legal order. The validity of laws is tested not by their psychological, sociological, or political consequences, but purely "normatively." (*The Pure Theory of Law*, 2nd ed., Berkeley, University of California Press, 1967; *General Theory of Law and State*, Cambridge, Harvard University Press, 1945.)

Various treatments of law by those influenced by recent language philosophy also have been prominent, with the views of H.L.A. Hart perhaps being the most important. (*The Concept of Law*, Oxford, Clarendon Press, 1961.)

Historical Jurisprudence. This often is listed as a separate school, the objectives of which are to trace the histories of legal systems, to interpret the changes that have taken place in laws, and to ascertain the influences that account for those changes. The methods used are similar to those used in other areas of historical inquiry. Many historical jurists believe that laws originate in customs, and they appraise the purposes and functions of contemporary laws in terms of their original purposes and functions.

Friedrich Karl von Savigny is generally acknowledged as the founder of the historical school. Law, he maintained, was a spontaneous emanation from the "life and spirit of a people," much like language, customs, and songs. The leading English exponent of this school was Sir Henry Maine. He asserted that legal development has been a movement from *status* to *contract*; i.e., a progression from a fixed condition in which an individual finds himself as part of a group to a system based upon contract.

One branch of historical jurisprudence is sometimes called the *comparative school* (Frederick W. Maitland, Frederick Pollack, Paul Vinogradoff, etc.). Historical methods are used to study two or more legal systems in an attempt to discover their similarities and differences.

Legal Realism. The legal realists view their subject matter not as a body of rules, the commands of a sovereign, etc., but as the behavior of court and other public officials. The realists, like the legal sociologists, inquire into the way laws function in practice, the economic and social influences on legal systems, and the psychological characteristics of legislators and judges. The legal views of Oliver W. Holmes, Joseph W. Bingham, John Chapman Gray, Roscoe Pound, John Dewey, and A.F. Bentley considerably influenced the realists. Karl Llewellyn, Edward S. Robinson, Thurman Arnold, Jerome Frank, Fred Rodell, and Felix Cohen are among the recent advocates of legal realism.

Often considerable emphasis is placed on predicting what the courts will do; the legal realist:

> "...seeks to isolate the factor or factors upon which court decisions actually rest in order to provide the student and the practitioner with an accurate method of forecasting what the courts actually do. The ideal is a 'science of law.' " (Fred V. Cahill, Jr., *Judicial Legislation: A Study in American Legal Theory*, New York, Ronald, 1952, p. 114.)

In *Jurisprudence: Realism in Theory and Practice* (Chicago, University of Chicago Press, 1962), Llewellyn summed up his position. He scrutinized claims that legal decision making is merely an outcome of a logical process, with the legal rule as major premise and the conclusion a product of deductive inference. He maintained that the realities of legal practice and decision making are far different. Law is not an abstract rule-system, but a going social institution that involves: (a) settling grievances and disputes, (b) channeling conduct in tension ridden situations, (c) redirecting new lines of conduct, (d) allocating decisional authority, (e) unleashing incentive, and (f) developing effective legal techniques for doing legal jobs.

In general, the realists tend to agree upon the following:

(1) Rejection of natural law.

(2) Insistence that inquiry into law must be based on empirical data, rather than on alleged transcendental concepts and intuitions. (Lee Loevinger, "An Introduction to Legal Logic," *Indiana Law Journal*, Summer, 1952.)

(3) Skepticism of the traditional use of deduction in jurisprudence. This skepticism takes several forms. Some doubt that a workable body of detailed rules of the type found in legal systems can be deduced from a relatively small number of "natural laws." As Jerome Frank pointed out, those who use syllogisms in their quest for legal certainty often forget that their premises may be doubtful. (*Law and the Modern Mind*, New York, Tudor, 1936, p. 66.)

(4) Emphasis on the transactions of those involved in legal processes, rather than on formalistic aspects of law. (Edwin N. Garlan, *Legal Realism and Justice*, New York, Columbia University Press, 1941, p. 5.)

(5) A desire to be scientific. Karl N. Llewellyn, for example, says that law is a body of data to be investigated in the same general way as inquiry is conducted in the physical sciences. ("Some Realism about Realism: Responding to Dean Pound," *Harvard Law Review*, Vol. 44, 1931.)

Sociological Jurisprudence. Sociological jurisprudence has many similarities to legal realism. Ludwig Gumplowicz and Eugen Ehrlich originated sociological jurisprudence; Roscoe Pound was the most prominent exponent in the United States. Other well known advocates have been Supreme Court Justices Benjamin N. Cardozo and Harlan Fiske Stone. Jurisprudence is regarded either as a social science or as continuous with, and dependent upon, the social sciences.

The legal order, according to Pound, is not concerned with abstract rights, but with actual interests, claims, and demands. Pound severely criticized traditional jurisprudence:

> "There has been too much abstract reasoning from attractive analogies of the past and not enough testing of those analogies in the light of how they meet or fail to meet the exigencies of reasonable expectations of men in the time and place. Abstract ethics and abstract politics must be supplemented by comparative study of the social and economic conditions from which their abstract theories are derived and of those to which they are to be applied. This is the point to which the pragmatism of Holmes and James is directed." (*Law Finding Through Experience and Reason*, pp. 47-48.)

Pound emphasized law both as a contributing force to, and a reflection of, society. Law was viewed as an instrument of social change, not as a body of fixed, deductively derived principles. He described the impact of sociology on jurisprudence as follows:

> "In the past fifty years the development of jurisprudence has been affected profoundly by sociology. The older mechanical sociology affected the science of law by its insistence upon thinking about groups. Thus it had much to do with bringing us to give up the abstract individual as the central point in juristic thought. Also this insistence upon a social theory led jurists to seek to relate law more critically to other social phenemena. Later the biological sociology brought about more thorough study of primitive legal institutions and gave impetus to the unification of the social sciences by establishing connections with anthropology and ethnology. Still later the psychological sociology gave us a more adequate account of the traditional element in legal systems, turned attention to the problem of judicial and juristic method, and made us aware of the traditional art of the lawyer's craft as an element in law and a factor in legal development." ("Sociology and Law," in W.F. Ogburn and A. Goldenweiser, eds., *The Social Sciences and Their Interrelations*, Boston, Houghton Mifflin, 1927, p. 324.)

Experimental Jurisprudence. According to Beutel:

> "...the steps employed in prosecuting a method of Experimental Jurisprudence should be approximately as follows:
>
> 1. The nature of the phenomena which law attempts to regulate should be studied. In particular, the social problem to which a specific law is

> directed should be carefully isolated and examined.
>
> 2. The rule of law or other method used to regulate the phenomena or intended to solve the social problem should be accurately stated.
>
> 3. The effect on society of adopting the rule should be observed and measured.
>
> 4. There should then be constructed a hypothesis that attempts to explain the reasons for this reaction.
>
> 5. This description, when broadened to apply to other analogous situations, might be considered a jural law that describes or predicts results which would occur on application of a similar regulatory law to similar problems.
>
> 6. If analysis shows that the law is inefficient, there could then be suggested new methods of accomplishing the originally desired result.
>
> 7. The proposed new law could be enacted and the process repeated.
>
> 8. A series of such adoptions of new laws and the study of their results might throw important light upon the usefulness of the underlying purposes behind the enactment, thus effecting a possible alteration in or abandonment of this objective, or in the long run, though this now appears doubtful, even induce a revision of our present scale of social and political ethics." (*Some Potentialities of Experimental Jurisprudence as a New Branch of Social Science*, pp. 18-19.)

In a later book Beutel says that a "jural law" is not a "man-made enactment to govern the conduct of men," but a "scientific law" or "warranted assertion." (*Democracy or the Scientific Method in Law and Policy Making*, Rio Piedras, University of Puerto Rico Press, 1965, pp. 98, 99.)

Although Beutel is the person chiefly associated with "experimental jurisprudence," other writers also have defended some version of experimentalism. See, for example, Thomas A. Cowan's "The Relation of Law to Experimental Social Science" (*University of Pennsylvania Law Review*, Vol. 96, 1948), and his "Experimental Jurisprudence and the Pure Theory of Law" (*Philosophy and Phenomenological Research*, Vol. 12, 1950).

Jurimetrics. According to Lee Loevinger, "the activities of those who practice jurisprudence and those who practice science are so unlike that there is very little communication and no exchange of work between them." He believes that the label "jurisprudence" is so closely associated with traditional procedures of philosophical inquiry that little progress can be expected even in "experimental jurisprudence"; he suggests that a new label, "jurimetrics," be used for scientific inquiry into legal processes. Loevinger and his co-workers are keenly interested in some of the newer developments in electronic data storage and retrieval, and they have great hopes for the application of symbolic logic to legal inquiry:

> "Jurimetrics is concerned with such matters as the quantitative analysis of judicial behavior, the application of communication and information theory to legal expression, the use of mathematical logic in law, the retrieval of legal data by electronic and mechanical means, and the formulation of a legal calculus of legal predictability." ("Jurimetrics: The Methodology of Legal Inquiry," in Hans W. Baade, ed., *Jurimetrics*, New York, Basic Books, 1963, pp. 7-8.)

4. RESULTS ACHIEVED

Much of the work done by jurists is deliberately nonscientific in method and intent, especially when inquirers believe that some absolute norms for human behavior exist and can be discovered through "reason," "insight," or "revelation." A vast literature of conjectures and conclusions about legal problems and issues, based on the type of procedures we called *self-actional* in Chapter I, is available. Much work in natural law "theory" falls into that category, as well as many of the conjectures of the analytic and the historical jurists.

Although recent work following self-actional procedures is less grandly metaphysical than earlier work, many inquirers are convinced that important principles can be derived from an "analysis of concepts." John Rawls, for example, in his influential book, *A Theory of Justice* (Cambridge, Belknap Press, 1971), develops principles of justice that supposedly would be accepted by "free" and "rational" persons under ideal conditions of "complete equality," and he also extends his argument to less ideal circumstances. (For other examples of the procedures used by recent analytic philosophers, see Robert S. Summers, ed., *More Essays in Legal Philosophy*, Berkeley, University of California Press, 1971.)

Considerable work has also been done in jurisprudence by sociologists, political scientists, and lawyers who follow the procedures we called *interactional* in Chapter I, sometimes with an admixture of self-actional procedures. Such work is partly "empirical," or observationally-based, but is guided by "normative" principles. Philip Selznick, for example, has considered labor-management collective bargaining processes in the light of his views on "authority," "legitimacy," and similar categories. (*Law, Society, and Industrial Justice*, New York, Russell Sage, 1969.)

In an attempt to make jurisprudence more scientific, cooperative research projects involving workers from several behavioral fields have inquired into the operation and social effects of specific laws and legal institutions. Walter Cook and his associates at John Hopkins pioneered in such endeavors, but the project failed for lack of funds during the Depression. For a number of years, Sheldon and Eleanor Glueck, a jurist and a sociologist, aided by a staff of physicians, anthropologists, social workers, and others, studied criminal behavior. A series of publications on adult and juvenile delinquency resulted, and the predictive tables developed have been used with some success in the treatment of offenders. (See Sheldon Glueck and Eleanor Glueck, *Toward a Typology of Juvenile Offenders*, New York, Grune and Stratton, 1970, for a recent example of their work.)

The Chicago Jury Project, another joint effort of jurists and behavioral scientists, is concerned with the deliberations of juries. (See Hans Zeisel, Harry Kalven, Jr., and Bernard Buchholz, *Delay in the Court*, Boston, Little, Brown, 1959; Harry Kalven, Jr. and Hans Zeisel, *The American Jury*, Boston, Little, Brown, 1966.) Other inquiries making use of the findings in several areas of research exist; sometimes associated with particular institutions, such as the Columbia University Project for Effective Justice, and sometimes not so institutionalized (e.g., Frederick K. Beutel, *Study of the Bad-Check Laws in Nebraska*, Lincoln, University of Nebraska Press, 1957). For a reasonably representative example of work attempting to relate law and the behavioral sciences, see the textbook by Lawrence Friedman and Stewart

Macaulay, *Law and the Behavioral Sciences* (Indianapolis, Bobbs-Merrill, 1969). Also of interest is the essay by Kenneth Culp Davis, "Behavioral Science and Administrative Law" (*Journal of Legal Education*, Vol. 17, 1965).

In recent years the topics mentioned below, among others, have received considerable attention.

There have been numerous studies of issues concerning compliance with laws, including the impact of laws and the resultant behavior. The book edited by Samuel Krislov *et. al.*, contains relevant materials from many behavioral fields (*Compliance and the Law: A Multi-Disciplinary Approach*, Beverly Hills, Sage, 1972).

Comparative and developmental studies have been made by anthropologists and others. E. Adamson Hoebel's work has been influential (*The Law of Primitive Man*, New York, Atheneum, 1968). At times the emphasis is on how disputes in general (not just legal conflicts) are handled in various cultural and social contexts, as in the volume edited by Laura Nader (*Law in Culture and Society*, Chicago, Aldine, 1969). Various comparative studies have been made; e.g., a cross-cultural study of judicial behavior (Glendon Schubert and David J. Danelski, eds., *Comparative Judicial Behavior*, New York, Oxford, 1969), and work on Indian law (Marc Galanter, "The Displacement of Traditional Law in Modern India," *Journal of Social Issues*, Vol. 24, 1968). Work also has been done in developing conjectures about possible "culture-transcendent" legal frameworks (Adda B. Bozeman, *The Future of Law in a Multicultural World*, Princeton, Princeton University Press, 1971).

Numerous studies have been made of police behavior, the discretionary power of police and prosecutors, sentencing, and related matters. For a sampling of the literature, see: James Q. Wilson, *Varieties of Police Behavior* (Cambridge, Harvard University Press, 1968); William A. Westley, *Violence and the Police* (Cambridge, M.I.T. Press, 1970); Abraham S. Blumberg, *Criminal Justice* (Chicago, Quadrangle, 1970); John Hogarth, *Sentencing as a Human Process* (Toronto, University of Toronto Press, 1971); and Kenneth Culp Davis, *Discretionary Justice: A Preliminary Inquiry* (Urbana, University of Illinois Press, 1971).

As was noted in the Chapter on Political Science, much attention has been given to judicial voting behavior, including Supreme Court Decisions, comparative studies of different types of courts, etc. Other topics of recent interest have been poverty law; work on contemporary social issues that have been the subject of Presidential Commissions (e.g., on Crime, Violence, and Obscenity); and studies of socialization, such as the attitudes of children toward the law, the professional socialization of lawyers and the police, etc.

Jurists interested in jurimetrics, and others, have been concerned with the possible applications of decision theory, computer techniques, information retrieval, and systems engineering in solving legal problems. (See Thomas A. Cowan, "Decision Theory in Law, Science, and Technology," *Science*, Vol. 140, 1963.) Game theory has been strongly emphasized by some workers, such as Glendon Schubert.

Many other areas of inquiry could also be mentioned, but the foregoing discussion indicates some of the work done in the field. Much of the recent work that is labeled "behavioral" emphasizes formal models and deductions from those models. Of interest also is the revival, partly in response to the emphasis on behavioralism, of "normative" procedures that tend to follow traditional patterns of inquiry.

5. CONTEMPORARY CONTROVERSY

In view of the range of opinion found in jurisprudence, a large number of controversies have occurred. We have selected for discussion in this Section a few controversies that relate to the main themes of this volume.

Underlying many of the specific controversies in jurisprudence are basic questions about which procedures of inquiry are most useful. As was noted earlier, some jurists regard their subject matter as primarily nonscientific; although they agree that useful information about the efficiency of means and the probable results of various courses of action can be obtained through scientific procedures of inquiry, they believe that the desirability of the basic *ends* or *norms* involved can be ascertained only in some other way (through reason, intuition, revelation, etc.). Other jurists argue that the entire field can be inquired into scientifically, but disagree as to which procedures of inquiry are scientific. At present, perhaps the largest number of those supporting "scientific method" in jurisprudence are convinced of the usefulness of developing formal models or other elaborate conjectures early in the inquiry, and then attempting to test those conjectures. Others, apparently far fewer in number, support procedures something like those suggested in Chapter I, in which there is a continual interweaving of observation and conjecturing.

Many of the issues just mentioned are involved in controversies over value judgments ("ought"-"is"; positive vs. policy science). Some who follow traditional procedures regard jurisprudence as "not concerned with problems of hypothesis and verification" and therefore as fundamentally unlike scientific inquiry (see quotation from O'Connell in Section 2). Among those sympathetic to the use of behavioral science procedures of inquiry in jurisprudence, marked differences of opinion are found about values. To illustrate, Pound and others regarded the social sciences as fundamentally concerned with what "ought-to-be" and as thus sharply differentiated from the natural sciences. Other writers defend "value-free" procedures of inquiry, as did the sociologist, George A. Lundberg:

> "The idea of Pound, and many others, of a fundamental difference between the physical and the social sciences in this respect. . .must be rejected as based on a confusion regarding science and the applied arts. More particularly the confusion rests upon the failure to recognize that practice and applied jurisprudence stand in the same relationship to the social sciences as physical engineering stands in relation to physics and chemistry. There is, of course, no reason why those social behaviors that have to do with legal and judicial processes should not be studied in strictly scientific fashion as the sociology of law, or the science of jurisprudence—an interstitial science combining parts of sociology, psychology, anthropology, economics, and political science. Such development is in fact to be expected, and encouraged." ("Conflicting Orientation in Law and National Policy," in Richard W. Taylor, ed., *Life, Language, Law: Essays in Honor of Arthur F. Bentley*, Yellow Springs, Antioch Press, 1957, p. 188.)

Beu[illegible] who perhaps is foremost among recent jurists in arguing for a scientific jurisprudence, believes that the "value" of legal ends or purposes can be inquired into (at least partially) by scientific procedures:

> "A system of Experimental Jurisprudence in its complete workings would not necessarily be limited to studying the efficiency of various legal devices in accomplishing their purpose. It could also throw considerable light on *the value of the purposes themselves*. . . . There is reason to believe that here, and in many other fields, scientific jurisprudence could become the instrument of experimental ethics in developing a new and finer civilization." (*Some Potentialities of Experimental Jurisprudence as a New Branch of Social Science*, p. 32, p. 36. Italics added.)

Perhaps the controversy can be adequately summarized as follows: There is considerable agreement that the operation of laws and related institutions can be described usefully using scientific procedures. Many, probably most, jurists are concerned also with the improvement of existing laws and legal institutions (the problem of "good laws," as traditionally stated). Some see such questions as beyond the scope of scientific procedures of inquiry; some argue for an expansion of the type of inquiry successfully used in the physical and physiological areas so that values can be encompassed; and some maintain that the same general procedures used by physical and physiological scientists can be used to choose among possible values. The controversies are exacerbated by the many inconsistent and incoherent applications of the name "value."

The claims made by a school of thought about its present or probable future achievements often are said to be exaggerated by the opponents of that school. Those supporting scientific procedures sometimes regard traditional "theories" as moribund, as naive, as dangerously inadequate for coping with present problems, or as semantic delusions. On the other hand, traditionalists often claim that the alleged scientific procedures are trivial, internally inconsistent, more like science fiction and numerology than genuine science, and lead to authoritarianism. (For some examples of such criticisms, see Lundberg, *op. cit.*, and Walter Berns, "Law and Behavioral Science," in Hans W. Baade, *op. cit.*)

Another set of controversies concerns *prediction*. Oliver Wendell Holmes' famous statement that the law consists in nothing more than "prophecies of what the courts will do in fact" ("The Path of the Law," *Harvard Law Review*, Vol. X, 1897, p. 461), has been followed by many similar comments. Walter W. Cook's 1924 statement could be accepted by many recent American jurists:

> "When a lawyer is confronted with what we call a legal problem, what he wishes to know, in order to reach a solution, is how certain governmental officials—judges and others—will behave when confronted by the given situation. He, as much as the physical scientist, is therefore engaged in trying to prophesy future physical events. In place of the behavior of electrons, atoms or planets, he has to deal with that of human beings. If he wishes to make a reasonably accurate prediction, it becomes necessary for him to examine into the past behavior of certain human beings—judges and similar officials—in like or similar situations—and it is on the basis of such study that he makes his prediction." ("The Utility of Jurisprudence in the Solution of Legal Problems," in Jacob Marks *et al., Lectures on Legal Topics, 1923-24*, New York, Macmillan, 1928, p. 337.)

Some of the recent disagreements about prediction can be usefully discussed in relation to what has been characterized as "the miracle of Rodell." Some eleven days before the *Baker v. Carr* decision (one of the most important cases to reach the Supreme Court during the period involved), Fred Rodell predicted not only the general outcome correctly, but also correctly predicted seven of the eight votes cast, and was not far wrong in some other specific predictions concerning the decision. ("For Every Justice, Judicial Deference is a Sometime Thing," *Georgetown Law Journal*, Vol. 50, 1962.)

Rodell opposed many of the views of judicial behavioralists about predicting behavior, and maintained that it was impossible to predict *precisely* because of the "infinite variety of quirks and causes" characteristic of the "mind." However, he maintained, the votes of Justices can often be predicted *in general* by studying the Justices as "whole human beings," including their "temperament, background, education, economic status [and] pre-Court career." (*Ibid.*, p. 708, pp. 700-701.)

The behavioralists argued that Rodell's impressive prediction probably was not replicable by others who might attempt to follow his procedures. According to S. Sidney Ulmer:

> "Rodell correctly predicted seven of the eight votes cast—an impressive result indeed. But in a larger perspective, it may be meaningless. For who knows how Rodell reached his result? Can any lawyer replicate Rodell's experiment and results? The important question is not whether one can predict judicial votes in one case by intuition or sheer guess, or through personal contact with judges or their clerks, but what replicable procedures are significantly successful over a long run of cases. If Rodell has discovered any successful predictive device based on 'human factors,' he has yet to make it available to the profession at large." ("Quantitative Analysis of Judicial Processes: Some Practical and Theoretical Applications," in Hans W. Baade, *op. cit.*, p. 165.)

And Glendon Schubert, in his criticism of Rodell, maintains that "theoretical knowledge," not just the skill of an excellent practitioner, is required in scientific inquiry:

> "The sophisticated lawman, in the tradition of legal realism, predicts on the basis of his specialized empirical knowledge; while the behavioralist, in the tradition of modern science, predicts on the basis of his theoretical knowledge." ("Judicial Attitudes and Voting Behavior: The 1961 Term of the United States Supreme Court," in Hans W. Baade, *op. cit.*, p. 142.)

Even among those who believe that prediction is a major part of jurisprudential inquiry, then, we find important disagreements based on conflicting notions of scientific procedures. Controversies of the type we have discussed probably will perpetuate themselves for a long time to come, in view of the disagreements about procedures of inquiry and the widespread semantic confusion in the field.

6. TERMINOLOGICAL PROBLEMS

Strong criticisms of the terminology used both by legal practitioners and by jurisprudential methodologists can be found. For example, in the 1920's, Walter W. Cook said:

> "One function of jurisprudence. . .is to examine critically the terms used in the lawyer's statements of his rules and principles, and the concepts for which these terms stand. . . . The first thing that strikes one is that down to date the members of the legal profession have in the main, both in England and in America, refused to make such a critical examination. The second is, that writers on jurisprudence who have done so have failed to exercise much influence upon the bench and bar. . . . Whatever the reason, we find such words as 'law,' 'right,' 'duty,' and so forth, used by the legal profession generally without a real attempt to discover what is meant by them." (Cook, *op. cit.*, p. 338.)

In the 1940's, Lee Loevinger criticized the unproductive circularity of legal "definitions":

> "[Legal thinking] proceeds like a dancing mouse chasing its own tail, moving rapidly and gracefully but staying on the same spot no matter how rapidly it moves. For example, a contract is defined in law as an enforceable promise or agreement. But when the question arises whether a particular promise is enforceable, it can be determined only by deciding whether or not that promise is a contract. One of the leading legal encyclopedias defines a contract as 'an agreement which creates an obligation.' It then proceeds to explain that 'the existence of such an obligation is essential to the existence of a contract,' and further clarifies the matter by saying that the obligation arises from the agreement as a 'duty imposed by law.' . . .The mental process is the same for practically all legal definitions. The pattern of legal thinking is the merry-go-round. 'A contract is an enforceable promise; a promise is enforceable if it is a contract.' 'Property is the right to exclude others from the use of a thing; you can exclude others from the use of a thing if it is your property.' 'Negligence is doing something a reasonable man would not do; a reasonable man is one who is not negligent.' So runs the wisdom of the law." (*The Law of Free Enterprise,* New York, Funk & Wagnalls, 1949, pp. 88-89.)

Later Loevinger noted the many conflicting applications of some of the leading names used in discussions of the procedures of legal inquiry:

> "The terms 'science' and 'law' have both been used for so long by so many writers with such a variety of meanings, clear and unclear, that one who aspires to clarity or rigor of thought or expression might well hesitate to use either one. . . . Exhaustive reading is not required to establish that there is neither an authoritative nor a generally agreed definition for any of the terms 'jurisprudence,' 'science,' or 'law.' " ("Jurimetrics: The Methodology of Legal Inquiry," p. 5.)

Many terminological disagreements are closely related to fundamental disagreements about basic procedures of inquiry, as is illustrated by the diverse applications of the name "law." Something of the range of application is suggested by the following quotations:

> ". . .the law. . .may be defined as follows: The sum total of the rules of conduct laid down, or at least consecrated, by civil society, under the sanction of public compulsion, with a view to realizing in the relationships between men a certain order--the order postulated by the end of the civil society, and by the maintenance of the civil society as an instrument devoted to that end." (Jean Dabin, *General Theory of Law,* in Kurt Wilk, trans., *The Legal Philosophies of Lask, Radbruch and Dabin,* Cambridge, Harvard University Press, 1950, p. 234.)

> "Law is a body of ideal principles and precepts for the adjustment of the relations of human beings and the ordering of the conduct in society. Law seeks to guide decisions as laws seek to constrain action." (Pound, *Law Finding Through Experience and Reason,* p. 1.)

> "The prophecies of what the courts will do in fact, and nothing more pretentious, are what I mean by the law." (Oliver Wendell Holmes, "The Path of the Law," *Harvard Law Review,* Vol. X, 1897, p. 461.)

> "[The experimental jurist takes law] simply as a man-made set of rules or controls which can be changed to conform to the dictates of scientific discoveries. He. . .concentrates. . .upon how it and the institutions surrounding it function." (Beutel, *Some Potentialities of Experimental Jurisprudence as a New Branch of Social Science,* pp. 16-17.)

"Justice" also has been applied to a wide range of phenomena; many of the discussions are based on conflicting philosophical points of view. (For a recent sample of such philosophical analyses, see "Symposium: *A Theory of Justice* by John Rawls," *The Journal of Philosophy,* Vol. LXIX, 1972.)

"Crime" also has been applied in diverse ways, ranging from "definitions" based on moral, social, and theological conjectures to statements that "crime" simply designates behavior prohibited by a legal code. Much the same variation is found for other widely-used labels claimed to represent key notions in jurisprudence.

Even when one moves away from the views that are overtly metaphysical or theological and considers allegedly behavioral inquiries in jurisprudence, terminological problems abound—many stemming from the use of procedures of the type called *self-actional* or *interactional* in Chapter I. For a discussion of such topics, see Section 6 in the Chapters on Sociology, Political Science, and Psychology.

7. COMMENT AND EVALUATION

A high degree of confusion prevails among some writers on jurisprudence about the procedures of scientific inquiry. Those trained as lawyers often have outmoded or inadequate notions about science, although perhaps that situation is improving. The recent behavioral scientists who write on jurisprudence frequently are enamored of elaborate formal models; their actual procedures are sometimes remarkably similar to the older philosophic procedures they officially reject. Throughout the field there is a strong tendency to begin inquiry with elaborate plausible conjectures that are assumed to be sound, and then to deduce consequences. Those consequences are sometimes tested by observation, but often are judged in terms of their apparent plausibility and by their agreement with other findings assumed to be warranted.

Some commentators regard law as a nonscientific area because actual statutes and judicial decisions are not arrived at in the same way that scientific assertions are. However, there are no theoretical or technological barriers of which we are aware that would preclude inquiry into legal institutions, processes, and behavior by the same procedures of inquiry scientists use in investigating other human behavior. Although the making, interpreting, and enforcing of laws has not usually been done by applying scientific procedures, the consequences of those laws and the procedures used in enacting and interpreting them can be studied scientifically. The system of laws of a given governmental unit can be examined as a formal system; the logical interrelation of laws can be investigated; alleged sanctions for law can be analyzed; but none of those activities excludes legal subject matter from scientific inquiry.

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9. GERMANE JOURNALS

ADMINISTRATIVE LAW REVIEW
AMERICAN BAR ASSOCIATION JOURNAL
AMERICAN JOURNAL OF LEGAL HISTORY
COLUMBIA JOURNAL OF TRANSNATIONAL LAW
CRIME AND DELINQUENCY
CRIMINOLOGY
HARVARD INTERNATIONAL LAW JOURNAL
JOURNAL OF LEGAL EDUCATION
JOURNAL OF PUBLIC LAW
JUDICATURE
JURIMETRICS JOURNAL
JURISPRUDENCE
LAW AND CONTEMPORARY PROBLEMS
LAW AND SOCIETY REVIEW
NATURAL LAW FORUM

There are also numerous journals devoted to general legal topics and published by law schools, such as the HARVARD, YALE, and COLUMBIA LAW REVIEWS. Many journals in Sociology and Political Science also publish articles on jurisprudence; see listings in those Chapters.

IX.

LINGUISTICS*

1. WORKING DESCRIPTION OF THE FIELD

LINGUISTIC scientists inquire into language structures, historical changes within languages, and the relations among languages. Considerable emphasis is placed on a language's system of sounds and the ways in which words and sentences are formed. Linguists often have focused their inquiries on the principles underlying the organization of languages rather than on the adjustive behavior of humans through language, but the latter also is investigated.

2. OTHER DESCRIPTIONS OF THE FIELD

Some writers emphasize linguistics as primarily inquiry into the structure of a language as a system of coding, rather than on the uses of language in adjustive behavior. John B. Carroll, for example, says:

> "Linguistic scientists are engaged in developing a sound body of scientific observations, facts, and systematic theory about language in general and about languages in particular. . . . But it must not be thought that linguistics is concerned with all phases of human communication. Instead, it narrows its attention to the study of languages conceived as what may be called 'linguistic codes.' A linguistic code may be regarded as a system of distinct sound symbols underlying the manifest speech behavior of the individuals comprising a speech community." (*The Study of Language*, Cambridge, Harvard University Press, 1953, p. 2.)

And Roger Brown says:

> "Linguistics aims at providing concepts that will serve to describe all languages and which can be used to contrast languages in regard to sound system (phonology), rules for word formation (morphology), and rules for word combination (syntax)." (*Words and Things*, Glencoe, Free Press, 1958, p. 22.)

Some writers emphasize the uses of language as also being part of linguistics. R.H. Robbins, for example, says:

> "Linguistics is quite simply the scientific study of human language in all its manifestations and uses, near and far, present and past, without restriction on time, place, or culture. The. . .student of linguistics studies languages, his own and foreign languages, as examples of mankind's faculty of language, to learn more about the way language works and how it may best be described and analysed." ("The Structure of Language," in Noel Minnis, ed., *Linguistics at Large*, New York, Viking, 1971, p. 15.)

The role of language in human cooperation may be stressed. Bernard Bloch and George L. Trager describe linguistics as the study of the "systems of arbitrary vocal symbols by means of which a social group cooperates." (*Outline of Linguistic Analysis*, Baltimore, Linguistic Society of America, 1942, p. 5.)

Typically language is regarded as a fundamental form of human behavior, particularly when human and nonhuman behavior are compared. Eugénie J.A. Henderson, for example, says that linguistics is the "scientific study of human language in all its manifestations, as one of the most important aspects of human behaviour, and perhaps the most characteristically human." ("Structural Organization of Language 1—Phonology," in Minnis, *op. cit.*, p. 37.)

3. METHODS AND TYPES OF INQUIRY

Humanists, grammarians, and philologists have traditionally dealt with the study of language. Linguistics as a science is a relatively new and small field. In the United States, descriptive linguistics has developed mainly since the first part of this century, stimulated by the work of Franz Boas and Edward Sapir on American Indian languages. The government-sponsored training programs in foreign languages during the World War II period helped to advance linguistic science in the United States.

"Natural science" procedures of inquiry were strongly emphasized until recently, when mentalistically-oriented procedures became prominent. Leonard Bloomfield's defense of behavioristic methods and his insistence that linguists avoid "meaning" as not capable of being investigated in an objective and precise way was highly influential for many years, but his general point of view has come under strong attack. (Leonard Bloomfield, *Language*, New York, Holt, 1933.) We will consider first the nonmentalistic, "natural science" phase of linguistics, and then move to the "revolution" associated with the work of Noam Chomsky.

Much work in linguistics emphasized spoken languages. E.H. Sturtevant, for example, says:

> "A language is a system of arbitrary vocal symbols by which members of a social group cooperate and interact. . . . The word *vocal* stands in the definition to exclude the human activities denoted by the phrases *gesture language*, *sign language*, *written language*, etc. All of these are important activities and proper subjects of investigation, and besides they have obvious connections with audible speech. The only reason for excluding them from our definition is convenience; they are found not to behave in the same way as audible language, and so they cannot conveniently to be treated scientifically at the same time." (*An Introduction to Linguistic Science*, New Haven, Yale University Press, 1947, pp. 2-3.)

One reason for the emphasis on vocal language is that some of the very early achievements (such as Grimm's Law in the nineteenth century) concerned regular shifts in sound from the Proto-Indo-European to the Proto-Germanic languages (e.g., the original consonant *t* in classical Latin changes to *th* in Germanic, the *th* in the

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*See the Chapter on Anthropology, where linguistics also is discussed.

English *three* corresponds to the *t* in the Latin *tres*). The orderly character of sound shifts, which occurred independently of grammatical functions and "meanings," impressed many observers.

Many other observed regularities can be described usefully in terms of their formal functions or other similarities and differences within a language, without regard to the specific "meanings" involved (i.e., what the signs, or names, are applied to) or the categories used in conventional grammar. Various language forms are grouped together because of their structural similarities within a language; e.g., in English *man* and *boy* are classed together because one can be substituted for the other to yield an utterance that is grammatically permissible according to the rules of English, not because of their specific "meanings" or because they are both nouns in traditional grammar. Our traditional grammatical categories (especially if viewed as reflecting universally necessary categories of thought) do not fit many languages. For example, the characteristic noun-adjective relation (in which adjectives designate qualities of "substances") found in Indo-European languages is not found in many other languages (including American Indian languages) that use an action verb-type expression; "the leaf is green" would be translated as something like "the leaf greens." To attempt to use our verb and adjective categories for describing such languages leads to a distorted and inefficient grammar. (For a brief discussion of such matters, see Joseph H. Greenberg, "Language and Linguistics," in Bernard Berelson, ed., *The Behavioral Sciences Today*, New York, Basic Books, 1963.)

More generally, many aspects of Indo-European languages are not found in other languages:

> ". . .nearly all the structural features of our language which we are inclined to accept as universal—features such as the actor-action sentence, the elaborate part-of-speech system, or the special inflections of our nouns and verbs—are peculiarities of the Indo-European family of languages and are by no means universal in human speech." (Leonard Bloomfield, *Linguistic Aspects of Science*, in *International Encyclopedia of Unified Science*, Vol. I, Part 1, University of Chicago Press, 1939, p. 221.)

Language often is viewed as a type of calculus in which the structuring of the elements, the "patterning," is the focus of attention. There then can be an indefinite number of calculi, having quite different structures and transformational rules; the objective of linguistic inquiry is to describe the formal structure of the languages that are or have been used. In recent years, as we shall discuss later, many linguists have been concerned with possible underlying ("deep") structures that are common to many (or all) languages, and there also have been many attempts to reinstate "meaning" as a proper concern of linguists.

To aid in the study of formal structures, linguists developed many technical devices and techniques of inquiry. Complex mechanical aids were used in the analysis and description of sounds:

> "The sound spectrograph is a device which transforms auditory into visual patterns. It readily and rapidly gives us information about the physical composition of speech sounds, which previously either was not obtainable or could be arrived at only through time-consuming calculations. Another device, the pattern playback, allows us to convert visible patterns into sound and makes it possible to evaluate the contribution of various features in making sound recognizable and understandable. X-ray sound motion pictures of the human vocal tract permit us to investigate in great detail the formation of the sound and to subject this event to physiological, physical, and psychological study." (John Lotz, "Linguistics: Symbols Make Man," in Lynn White, Jr., *Frontiers of Knowledge in the Study of Man*, New York, Harper, 1956, p. 218.)

Rather than viewing letters and words as the basic elements from which languages are composed, linguists have studied units such as the *phoneme* and *morpheme*. As will be discussed in Section 6, some disagreement is found as to what those names designate, but the following quotations illustrate the general way in which they are used:

> "A phoneme, then is either a single speech sound or a group of similar speech sounds, which in a given language function in the same way. . . . [A] phoneme is a minimum unit of distinctive sound-feature." (Sturtevant, *op. cit.*, pp. 15-16.)

> "The morpheme is a unit reminiscent of the word but not to be identified with it any more closely than the phoneme is to be identified with the letter of the alphabet. The morpheme is the minimal semantic unit. . . . A plural form like *dogs* is not a single morpheme word as it analyzes into the free form *dog* and the bound form *-s*." (Brown, *op. cit.*, pp. 50-51.)

> "The morpheme is the smallest element with which meaning can be associated. . . . In the following example the morpheme divisions are indicated by hyphens: The-cat-s-are-purr-ing." (Lotz, *op. cit.*, p. 218.)

Other presumed units, such as *tagmemes* and *semenes*, have also been differentiated; in general the principle used in much linguistic inquiry was to search for the smallest unit of a particular type, and then to investigate the ways in which those units were combined, much as physical scientists studied atoms (and later constituent parts of atoms) and their combinations.

The achievements of the earlier ("Bloomfieldian") phase of linguistics often were highly rated, and there seems to be no question but that the structures of many languages were described usefully. In a number of ways, however, dissatisfaction was expressed by observers both inside and outside the field. The range of language behavior investigated was narrow, the adjustive behavior of humans through the use of language was largely excluded, and in general the emphasis was on form or structure to the exclusion of the "functions" of language. The dissatisfaction was often expressed as a need for linguists to investigate "meanings."

Within linguistics, an influential change in procedures occurred in the late 1950's that involved an explicit mentalism and a revival of the doctrine of innate ideas as espoused by Descartes and Leibniz. Noam Chomsky, for example, maintained that "contemporary research supports a theory of psychological *a priori* principles that bears a striking resemblance to the classical doctrine of innate ideas," and George A. Miller says: "I now believe that mind is something more than a four-letter, Anglo-Saxon word—human minds exist and it is our job as psychologists to study them." (Chomsky, "Recent Contributions to the Theory of Innate Ideas," in Robert S. Cohen and Marx W. Wartofsky, eds., *Boston Studies in the Philosophy of Science,* Vol. III, New York, 1967, p. 81; Miller, "Some Psychological Studies of Grammar," in L.A. Jakobovits and M.S. Miron, eds., *Readings in the Psychology of Language,* Englewood Cliffs, Prentice-Hall, 1967, p. 217.)

The revival of mentalism in linguistics was a radical change; for years Bloomfield's rejection of mentalism was widely accepted by linguists as necessary for progress. Bernard Bloch described the earlier situation as follows, in his obituary of Bloomfield:

> "In his long campaign to make a science of linguistics, the chief enemy that Bloomfield met was that habit of thought which is called mentalism: the habit of appealing to mind and will as ready-made explanations of all possible problems. Most men regard this habit as obvious common sense; but in Bloomfield's view, as in that of other scientists, it is mere superstition, unfruitful at best and deadly when carried over into scientific research." ("Leonard Bloomfield," *Language,* Vol. 25, 1949.)

Chomsky argued, on grounds such as the fundamental similarity he believes is found in the grammatical structure of all languages and the speed with which a young child learns to construct sentences, that the principles of language must be innate rather than learned. The syntactic structures of a language are logically generated in accordance with the rules of a language, and the "mind" decides which combinations of sounds are permissible grammatically. The task of linguists is to make explicit the rules that connect sound and "meaning" for the infinite number of sentences that can be uttered ("generated") within a language. In carrying out that task, the "deep structure" (involving meaning) of the language is investigated as the underlying basis for the "surface structure" (the words making up the spoken sentence).

Chomsky was led to many of his views because traditional structural linguistic inquiries had difficulties with syntactical problems. To illustrate by an instance often mentioned, the sentences *John is eager to please* and *John is easy to please* appear to have the same structure, but are very different syntactically; the structuralist procedures encountered difficulty in adequately describing such differences. In addition, syntactically ambiguous sentences such as *I like her cooking* were troublesome for structural linguists. Chomsky believed such difficulties could be overcome by a doctrine involving different deep structures that were concealed by similar or identical surface structures. (See Chomsky's *Language and Mind,* New York, Harcourt, Brace & World, 1968; *Cartesian Linguistics,* New York, Harper & Row, 1966; *Aspects of the Theory of Syntax,* Cambridge, M.I.T. Press, 1965; and *Syntactic Structures,* The Hague, Mouton, 1957.)

Not long after Chomsky's revolution, his views were severely criticized by other linguists (including some of his own students) who, however, retained many of his procedures of inquiry. The "generative semanticists," including Paul Postal, John Ross, James McCawley, and George Lakoff, reject the sharp division drawn by Chomsky between syntax and semantics. They attempt to move from meaning to surface structure without invoking deep structures. In addition, generative semanticists emphasize the occurrence of utterances within an appropriate context; a grammar should include rules not only for properly formed sentences but for the setting (e.g., instances where we normally would preface a command by "please," contrasted to other contexts where a blunt command would be given).

Although the deviation of his students and others from Chomsky's views was accompanied by much heated controversy, the "rebels" share his general frame of reference. Compared to many earlier linguists who emphasized the desirability of a close connection between scientific conjectures and the evidence (sometimes expressed by linguists as the need for theorizing to be data-based), the newer group regards it as useful to elaborate highly speculative conjectures far in advance of the evidence. The comments by Paul Postal about a paper in which he attempted to derive some universally true statements concerning language on the basis of a comparison of Mohawk and English illustrate this clearly. In response to a critic who wondered about making "universal statements on the basis of contrastive study of only two languages," Postal replied:

> ". . .I would be willing to postulate universals on the basis of an even weaker study, namely of one language. . . . There seems to be a strange idea that one should play it safe and claim as little as possible. What's wrong with basing a universal hypothesis on two languages. If it's wrong, then dozens of people will immediately come forward and present the evidence. It is clear that the ideal of studying, say, a hundred or two hundred languages and then stating hypotheses is wonderful. The odds are that if one did that the hypotheses would be much more correct. But unfortunately no one has been able to do that. I see nothing wrong with basing hypotheses on the small number of languages which it is possible to study at all by one person, and letting those who have not studied those languages, but have studied other languages, determine whether these hypotheses are correct. I see no possible flaw in that approach. It seems to me that it's perfectly reasonable. So I can't imagine why you object to it." ("The Method of Universal Grammar," in Paul L. Garvin, ed., *Method and Theory in Linguistics,* The Hague, Mouton, 1970, p. 130.)

Other points of view than those just discussed also can be found. Although the earlier taxonomic procedures of the structural linguists, focusing on "item-and-arrangement," appear to be de-emphasized by most of the recent linguists, there are those who regard the revived mentalism so prominent in the field as fundamentally nonscientific and who wish to use the "empirical" methods of the earlier scientific linguists. Perhaps the chief representative of that point of view is Charles F. Hockett. (See his *The State of the Art,* The Hague, Mouton, 1968.) As in several other behavioral fields, those who most blatantly use mentalistic and dualistic procedures are likely to insist that they are being scientific and that criticisms of their procedures, such as those made in Chapter 1, simply reflect an unduly narrow view of scientific inquiry.

4. RESULTS ACHIEVED

Linguists have developed useful methods for describing and comparing the formal structures of languages, changes in those structures, and connections among languages. Both the techniques of inquiry used and the detailed statements of findings are highly technical. Some indication of the main areas in which linguists work is provided by Carroll's classification, although others would subdivide the field differently. Carroll noted the following "levels of analysis": *Phonetics*, the description of sounds in terms of articulatory or acoustic principles; *Phonemics*, the classification of sounds in terms of minimal units (phonemes); *Morphology*, the identification of morphemes, and inquiry into their arrangements in words and the morphemic changes that occur in different grammatical contexts; *Morphophonemics*, the investigation of phonemic variations that are found as morphemes occur in different grammatical contexts, and the constitution of morphemes from phonemes; *Syntax*, the investigation of the ways in which sentences are built up from words; and *Lexicography*, the search for the complete listing of all the "meaningful" elements in a language. (Carroll, *op. cit.*, pp. 24-25.) Stated generally, linguists have developed reasonably adequate descriptions of a large number of the world's languages, although some languages have not been described and existing descriptions are subject to further correction and improvement.

Structural linguists have developed basic procedures for identifying phonemes and morphemes and for describing the sound patterns of languages. To mention only one example, linguists have found that the number of phonemes in most languages varies somewhere between 15 and 50, and that the phonemes in a given language may be made up of combinations of even fewer distinctive phonological features, such as those of place and manner of articulation. The ability to control the various configurations of the vocal tract involved in the language's phonological form is an important part of learning a language. (See Robbins, *op. cit.*, pp. 23-24.) Much useful work has also been done on grammatical structures (written and spoken) and on lexicons.

The achievements of the structural linguists in their work on the formal structures of language (although much criticized by some of the recent linguists) frequently has been assessed highly. For example, in the mid-1950's Clyde Kluckhohn described linguistics as the behavioral science field that had achieved results most nearly resembling those of the physical scientists, and he suggested several reasons for the success of linguistic inquiry. Linguistic changes occur at a relatively slow rate, and often the data available are extensive; on "Indo-European, Semitic, and Sino-Tibetan languages there are available statistical runs of four to five thousand years." Inquiry in some areas of human behavior can be difficult because the inquiry may seriously modify the behavior being investigated, but formal language structures are not likely to be so modified. ("Anthropology," in James R. Newman, ed., *What Is Science?*, New York, Simon and Schuster, 1955, p. 346; pp. 349-350.)

The type of agreement reached by inquirers in descriptive linguistics (understood in terms of the formal structures as described above) is far less common in other areas, including historical or comparative linguistics and studies of language in general, where conjecturing often is done far in advance of the evidence. Recently, as we have noted, elaborate conjectures about human language behavior have become prominent in the field and also have been influential in several of the other behavioral science areas. Some of those issues will be discussed further in later sections of this Chapter.

Much work has been done that links together concerns of linguists and those of other behavioral scientists, such as anthropological linguistics (see Chapter on Anthropology). Psycholinguistics is an area with a considerable body of literature; the emphasis has been on such topics as how children acquire language, the relation of behavior disorders to speech behavior, the connections between psychological traits and linguistic patterns, and on the study of supposed universal features of the human "mind." (See Sal Saporta, ed., *Psycholinguistics*, New York, Holt, Rinehart and Winston, 1961; Roger Brown, ed., *Psycholinguistics: Selected Papers*, New York, Free Press, 1972; and Jakobovits and Miron, *op. cit.*)

Earlier work in sociolinguistics often reflected the views of Benjamin Whorf concerning the influence of language on culture:

> ". . .the background linguistic system (in other words, the grammar) of each language is not merely a reproducing instrument for voicing ideas but rather is itself the shaper of ideas, the program and guide for the individual's mental activity, for his analysis of impressions, for his synthesis of his mental stock in trade. Formulation of ideas is not an independent process, strictly rational in the old sense, but is part of a particular grammar and differs, from slightly to greatly, as between different grammars. We dissect nature along lines laid down by our native languages. The categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary, the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds—an agreement that holds throughout our speech community and is codified in the patterns of our language." (*The Technology Review*, Vol. 42, 1940, p. 231.)

More recent work has tended to reverse Whorf's emphasis and to investigate the influences on language of sociocultural factors, such as social status differences and sociocultural changes. Sometimes *languages* and *societies* are viewed as separate, interacting entities, but sometimes they are viewed at least partially in the manner we call transactional (see Chapter 1). Dell Hymes and John Gumperz particularly have argued against the methodological separation of language and society. (For work in this area, see William Bright, ed., *Sociolinguistics*, The Hague, Mouton, 1966; Joshua A. Fishman, ed., *Readings in the Sociology of Language*, The Hague, Mouton, 1968; Dell Hymes, ed., *Language in Culture and Society*, New York, Harper & Row, 1964; and John J. Gumperz, *Language in Social Groups*, Stanford, Stanford University Press, 1971.)

The findings and conjectures of linguists have been used in many areas, including the teaching of foreign languages, the teaching of English, the educational techniques used with children having reading difficulties, and the machine translation of languages, not always with the hoped for results.

5. CONTEMPORARY CONTROVERSY

In recent years the controversies within linguistics about procedures of inquiry often have been heated and sometimes are carried on in the personalized way characteristic of many polemics in philosophy and literary criticism.

Many of the controversies concern the work of Chomsky and others at M.I.T. Chomsky is sometimes regarded as an outstanding genius who has revolutionized the field; on the other hand, Robert A. Hall, Jr., maintains that the point of view developed at M.I.T. has set the field of linguistics back 400 years. (*An Essay on Language*, Philadelphia, Chilton, 1963, p. 149.) Chomsky has been praised for reuniting modern grammar and the traditional grammar of past centuries; one reviewer regards that as similar to saying that "an anthropologist had done us the 'special service' of reuniting Darwin and the Bible." (William M. Austin, review of *Linguistics Today*, in *American Anthropologist*, Vol. 72, 1970, p. 1150.)

An important aspect of the controversies stemming from the work of the transformationalists and the generative semanticists is the role of conjecturing in inquiry. The issues are often discussed using categories such as rationalism vs. empiricism and deduction vs. induction. Dwight Bolinger describes the disagreement as follows:

> "Unlike those linguists whose main business is to survey usage, who amass evidence and then attempt to set it in order inductively and formulate theories about it, the formalists take an early leap to their hypotheses and then test them deductively against the data." (*Aspects of Language*, New York, Harcourt, Brace & World, 1968, p. 210.)

Sometimes the issue is posed in terms of a distinction between *hypothetical-deductive* and *descriptive* procedures, with an elaborate conjectural apparatus or model being used during the initial stages of inquiry by those favoring the hypothetical-deductive method, but with conjectures emerging only "as a result of successive descriptive studies" in the other method. (See Madeleine Mathiot, "Theory-Building in the Descriptive Approach," in Garvin, *op. cit.*, especially pp. 159-160.) The participants in this controversy generally support the testing of conjectures; the differences relate to when the testing is done in the course of inquiry.

Another controversy concerns the general objectives of linguistic inquiry. Some emphasize the description of the formal structure of languages, while others emphasize the use of languages in the processes of human communication. This issue cuts across some of the controversies about procedures of inquiry. For example, in the 1940's John Dewey and Arthur F. Bentley criticized severely the epistemological views shared by many of the structural linguists. Dewey and Bentley rejected interactional procedures, in which presumed ultimate and separable units interact with each other, and protested strongly the attempted sharp separation of *syntax*, *semantics*, and *pragmatics*. (*Knowing and the Known*, Boston, Beacon Press, 1949, especially Chs. I and IV.) On the other hand, Dewey and Bentley supported the structural linguists' rejection of mentalism and the emphasis they placed on "data-based theory." The recent generative semanticists accept Chomsky's mentalism and *a priorism*, but reject his separation of syntax and semantics. They maintain that syntactic structure is shaped by semantic factors; what is a "well-formed" sentence may depend on semantic, as well as syntactical, factors (e.g., *Tom called Jane a pacifist, and then SHE insulted HIM* is "well-formed" only if Tom and Jane regard being called a pacifist as insulting).

Another set of controversies concerns the acquisition of language by children. Many of the younger, "rationalist" linguists maintain that the basic structure of language is innate and that only the details of the surface structure, which vary from language to language, have to be learned. Their "empiricist" opponents tend to argue that all parts of language are socially learned. Many discussions of this issue are handicapped by uncertainties as to what, if anything, "innate" designates. See Section 6. (For a discussion of this controversy, see Ronald W. Langacker, *Language and Its Structure*, New York, Harcourt, Brace & World, 1968; Renira Huxley and Elisabeth Ingram, eds., *Language Acquisition: Models and Methods*, New York, Academic Press, 1971.)

There also is controversy over the sharp distinction Chomsky and others make between *competence* and *performance*. Competence is somewhat Platonic; it refers to a person's awareness (tacit or more conscious) of the rules of language, independent of the social setting in which language is used. Performance refers to the actual use of language in a particular setting. Chomsky holds that we are born with the aptitude for acquiring the rules, and that this "explains" why children learn their native languages so quickly. Again we have the problem of what "innate" and "knowledge" designate. Some of the arguments used have a pre-Darwinian character reminiscent of old arguments for God's existence on the basis of observed design in the world. For example, Chomsky says that to "postulate" an "unconscious knowledge of the rules of grammar" is "empirically justified" by the usefulness of the postulate in "explaining" the "use and understanding and acquisition of language." ("Linguistics and Philosophy," in Sidney Hook, ed., *Language and Philosophy*, New York, New York University Press, 1969, p. 155.) If children were born with a knowledge of grammar, that would account for their early acquisition of language, but also, if a Great Designer existed, that would account for observed regularities in the world; if birds knew innately the principles of aerodynamics, that would account for their flying performance; and if tree leaves knew the requisite physics and mathematics to maximize their exposure to sunlight, that would account for the observed densities of leaves around a tree.

There are also disagreements as to whether some languages are more "primitive" or less developed than others. For some time there was considerable agreement that all languages are equally developed in the sense that all can adjust to the formulation of whatever notions the language users wish to discuss:

> ". . .there can be no 'better' or 'worse' language, no more highly developed language, no more 'primitive' language. It is obvious that one would be hard put to find a vocabulary for inside plumbing in a culture that had no inside plumbing. But once the blessings of modern technology are diffused to a people and integrated into the culture, we find no language that cannot perfectly adequately talk about the new cultural acquisitions. . . . What is important to one group is often considered laughably trivial in another. . . . The Australian [aboriginal] language abounds in kinship terms, and we manage with a relative handful of imprecise and

> cumbersome terms like 'cousin,' 'uncle,' 'brother-in-law.' It's not surprising that when European missionaries began working with the Australians, they were soon asked by the Aborigines how they managed with such a miserably developed language." (Henry L. Smith, Jr., "Linguistics," in L. Bryson, ed., *An Outline of Man's Knowledge of the Modern World*, New York, McGraw-Hill, 1960, pp. 349-350.)

More recently some writers have urged that some languages are evolutionarily more advanced than others. Brent Berlin, for example, notes that languages having only a few basic color names are spoken by peoples living on a relatively primitive economic and technological level, while languages with a larger color lexicon are spoken in technologically more advanced cultures. ("A Universalist-Evolutionary Approach in Ethnographic Semantics," in Ann Fischer, ed., *Current Directions in Anthropology*, Washington, D.C., American Anthropological Association, 1970, p. 14.) And Dell Hymes has argued:

> "I want to controvert two widely accepted views, first, that all languages are functionally equivalent, and second, that all languages are evolutionarily on a par. I want to maintain that the role of speech is not the same in every society and that the differences can best be understood from an evolutionary point of view; that we must understand speech habits as functionally varying in their adaptation to particular social and natural environments, and recognize that there are ways in which some languages are evolutionarily more advanced than others." ("Functions of Speech: An Evolutionary Approach," in F.C. Gruber, ed., *Anthropology and Education*, Philadelphia, University of Pennsylvania Press, 1961, p. 55.)

Some controversies to a considerable extent are disagreements about terminology, but also involve disagreements about facts and methods. For example, several conflicting views can be found about the range of behavior most usefully designated as *language*. One point of view emphasizes communication and adopts an evolutionary perspective that does not assume the existence of absolute differences "in kind" among the various types of communication. Perhaps the fullest statement of that point of view is by Dewey and Bentley. Although they conclude that sign-behavior is characteristically human and is found almost exclusively among humans, they view that behavior as evolving from subhuman modes of communication and as continuous with it. (Dewey and Bentley, *op. cit.*, especially Ch. VI.) Others see sharp separations, reminiscent of the Aristotelian notion of fixed species, rather than continuity. Often the issue is discussed in connection with "paralanguage" and "kinesics" (tone of voice, gestures, and bodily movements). According to William M. Austin: "Cats and dogs have paralanguage and we communicate with them in that modality, but language is an all-or-none proposition; there is no such thing as 'half' language." (Austin, *op. cit.*, p. 1151.) Sometimes the issue also is discussed in terms of what constitutes a "true" language. Edmund Leach, for example, says:

> "With true language, once we have learned the grammatical and phonological rules. . . , each individual is capable of making an indefinitely large number of brand-new utterances. . . . When we talk we use words and short phrases which we have often used before, but we are constantly stringing them together into new combinations and permutations. . . . Provided that you and I can speak 'the same language' I can make an utterance which has never been made before in the whole history of the world, and you will still understand what I am saying." ("Language and Anthropology," in Minnis, *op. cit.*, p. 141.)

6. TERMINOLOGICAL PROBLEMS

The reliance on "meaning," especially among the recent, "rationalistic" linguists, leads to considerable confusion. Often the emphasis on "meaning" is a part of the procedure we called *self-actional* in Chapter I; "meanings" are secretions, as it were, of a mind. At other times, as part of an *interactional* procedure, "meanings" are viewed as intermediate, connecting links between words and things. To a considerable extent, the recent focus on "meanings" is a reaction against the earlier behavioristic procedures used by linguists. We suggest that both the earlier rejection of "meanings" and the recent enshrinement of them reflect inadequate procedures of inquiry. The linguistic behaviorists, in their desire to be objective scientists, tended to exclude the characteristic adjustmental behavior of humans through the use of signs; while the "rationalists," in attempting to inquire into that behavior, adopted procedures that practically guarantee failure. (See Section 7, Psychology Chapter.)

"Theory" and "explanation," especially when a theory is said to explain observations and findings, lead to the type of difficulty discussed in Section 6 of the Chapters on Psychology and Anthropology. Rather than repeat those discussions, we will consider the matter here in connection with "innate." Supporters of "innatism" seem to differ as to just what it is that is innate. Sometimes, apparently, only a "predisposition" to, or aptitude for, certain types of behavior is intended; birds have the appropriate biological structure for flying, men for walking, etc. "Explaining" behavior in terms of innate aptitude for that behavior does not seem useful; to say that someone contributes heavily to charity because he is benevolent hardly moves inquiry ahead.

At other times, however, innate *knowledge* is stressed. Chomsky appears to maintain that children have an innate and unconscious knowledge of certain principles, such as the "principle of the transformational cycle." That complex principle involves the way in which phonological rules are applied in order to yield correct utterances in a language. According to Chomsky, children in all cultures and environments quickly learn to speak in accord with that principle, which is explained by the child's possession of innate knowledge of the principle. (Noam Chomsky and Morris Halle, *The Sound Pattern of English*, New York, Harper & Row, 1968, p. 43.) But again, such explanations do not seem to move inquiry forward. Because the planets move in a certain way does not imply that they know, consciously or unconsciously, the principles of their motion; because young children can produce an indefinite number of grammatically appropriate utterances does not imply that they know what the rules are. Obscurantism, rather than clarification, seems to be the major result of invoking "innate knowledge."

Turning now to terminological problems specific to linguistics, there have been some problems with both "phoneme" and "morpheme." The following are representative statements of what "phoneme" designates:

> "A phoneme is the sum of those sound features which are distinctive." (Lotz, *op. cit.*, p. 220.)

> "A grammatical abstraction to designate a class of equivalence of minimum speech-sound." (Joshua Whatmough, *Language: A Modern Synthesis*, New York, Mentor, 1956, p. 233.)

H.A. Gleason, Jr., gives three different "definitions":

> (a) "the minimum feature of the expression system of a spoken language by which one thing that may be said is distinguished from any other thing which might have been said."
>
> (b) the "class of sounds which: (1) are phonetically similar and (2) show certain characteristic patterns of distribution in the language or dialect under consideration."
>
> (c) "one element in the sound system of a language having a characteristic set of interrelationships with each of the other elements in that system."

He then goes on to say: "These three definitions. . .are complementary. No one of them gives a full picture of the nature or significance of the phoneme." (*An Introduction to Descriptive Linguistics*, New York, Holt, 1955, pp. 16, 162, 168-169.)

Henderson says:

> "Phonemes themselves are best thought of as abstract units which are realized as speech-sounds in utterances. The term 'phoneme' is not a synonym for 'speech-sound,' though it is sometimes used as such by laymen." (Henderson, *op. cit.*, p. 42.)

The variations in the statements of what "phoneme" designates result from attempts to make the descriptions of what is involved more consistent and coherent. Some of the difficulties encountered appear to stem from the assumption that phonemes are ultimate units. Recently there has been a tendency *not* to view phonemes as the smallest, "unsplittable" phonological units, and to search for components within phonemes. (See Henderson, *op. cit.*, pp. 46ff.)

Although there is considerable agreement among linguists as to what the morphemes of a given language are, statements as to what "morpheme" designates are not always clear or mutually consistent. Often morphemes are said to be the smallest units of a language that have meaning. This involves one in all the problems of the "meaning of meaning." Probably to avoid such problems, H.L. Smith says: "the term 'morpheme' designates a *class* of related, recurring events." (*Op. cit.*, p. 362.) Perhaps the similarity among the members of the class can best be described in terms of the significance the morphemes have for the users of a language. For example, pluralness as represented by *s* and the continuing process represented by *ing* are significant to users of English. As aspects and phases of things and events are differentiated, they are designated by morphemes and combinations of morphemes.

7. COMMENT AND EVALUATION

A strong commitment to presumed scientific procedures characterized the field of linguistics for some time, but more recently great dissatisfaction developed about the results achieved, followed by the use of procedures of inquiry that in many respects are medieval. We suggest that the interactional procedures so prominently used by the linguists in the 1950's had serious limitations, even for the description of the formal structures of languages. Unfortunately, the critics of the procedures used in the 1950's regressed to self-actional procedures, rather than moving forward to transactional procedures.

As we have noted, many of the earlier linguists took Newtonian physical inquiry as their model. They searched for ultimate, unanalyzable, "linguistic atoms," which were then brought together in mechanical interaction. Just as Newtonian physics was successful within a certain range, so the structural linguists were successful; but just as the Newtonian procedures failed for other problems, so did the procedures used by the structural linguists.

We do not intend to minimize the achievements gained through the use of interactional procedures. Useful descriptions of many of the world's languages, especially the "crystallized" aspects involved in encoding and decoding, resulted. On the other hand, the results concerning language structure were not particularly useful for inquiry into the adjustive behavior of humans through the use of language, which many workers in many different fields deemed important. (To mention only two examples, Arthur F. Bentley discussed the importance of developing a "general theory of language" in his *Inquiry into Inquiries*, Boston, Beacon Press, 1954, Ch. 13; and J.R. Kantor advocated a scientific inquiry into language uses that would not view language as "motor action expressing psychic processes called thoughts" in his *The Logic of Modern Science*, Bloomington, Principia Press, 1953, pp. 265-267, 302.) In addition, difficulties emerged even within the inquiries into formal structures; phonemes, for example, did not behave as ultimate "atoms" were expected to behave.

Of particular significance is the attempted sharp separation of signs, sign-users, and what is signified. Rather than inquiring into the entire sign-process transaction as ongoing behavior, most linguists of both the older and newer points of view assumed that two separate realities are somehow brought together by a third entity. To illustrate, the editors of a recently established journal, *Semiotica*, say in the material released about their journal:

> "A sign, by all accounts, from Stoic philosophy to contemporary thinking, is conceived as a necessary coupling between twin moieties: the signifier, a perceptible impact on at least one of the sense organs of the interpreter, and the content signified. This twofold structure of perceptible and intelligible is the indispensable criterion for the division of signs into such types as signal, symptom, icon, index, symbol, emblem, and name, which constitute a principal focus of current semiotic researches."

The harmful consequences of such separations were analyzed in detail by Dewey and Bentley in *Knowing and the Known*. Of immediate interest is their statement (in 1949) of what happens when an Aristotelian procedure emphasizing rules, definitions, fixed categories, etc., is followed:

> "All theories of linguistics, at least with a rare exception or two, make their developments along these lines. In the region of characterization the

> view arises that if naming occurs there must be a 'some one' to do the naming; that such a 'some one' must be a distinctive kind of creature, far superior to the observed world—a creature such as a 'mind' or personified 'actor'; and that for such a 'some one' to give a name to 'anything,' a 'real' thing or 'essence' must exist somewhere apart and separate from the naming procedure so as to get itself named. . . Alien as this is from modern scientific practice, it is, nevertheless, the present basis of most linguistic and logical theory and of what is called 'the philosophy of science.' " (*Knowing and the Known*, pp. 159-160.)

The extreme mentalism found in current linguistic inquiry (possibly even more extreme than that described by Dewey and Bentley in 1949) perhaps will impede inquiry as much as mentalism has impeded inquiry in the past. What we believe is required for progress is not a return to the interactional procedures typical of the earlier structural linguists, but an advance to transactional procedures. (For a further discussion of those procedures, see the Appendix.)

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9. GERMANE JOURNALS

AMERICAN ANTHROPOLOGIST
ANTHROPOLOGICAL LINGUISTICS
COGNITION
CURRENT ANTHROPOLOGY
FOUNDATIONS OF LANGUAGE
GENERAL LINGUISTICS
INTERNATIONAL JOURNAL OF AMERICAN LINGUISTICS
JOURNAL OF THE ACOUSTICAL SOCIETY OF AMERICA
JOURNAL OF LINGUISTICS
JOURNAL OF PSYCHOLINGUISTIC RESEARCH
LANGUAGE
LANGUAGE SCIENCES
LINGUA
LINGUISTIC INQUIRY
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PSYCHOLOGICAL BULLETIN
PSYCHOLOGICAL RECORD
PSYCHOLOGICAL REVIEW
QUARTERLY JOURNAL OF SPEECH
SEMIOTICA
SOUTHWESTERN JOURNAL OF ANTHROPOLOGY
STUDIES IN LINGUISTICS
SYNTHESE
WORD

NOTE ON THE NEWER FIELDS*

THE subject matters of the older behavioral fields as conventionally differentiated often overlap, and many inquiries are conducted across disciplinary lines. In recent years considerable attention has been given to the development of formal models that supposedly can be applied to problem areas in many or all of the older behavioral fields, and often claims are made that even wider applications are possible. Although much of the basic work in game and decision theories, for example, was done by economists who believed that they had found new applications for mathematical procedures, that work often has been regarded as having considerable significance for psychology, sociology, anthropology, political science, international relations, and other fields. Work in information theory and cybernetics often originated in engineering contexts, but has been applied to many behavioral science fields. Much of the initial work in general systems theory was done by biologists; but, as the name suggests, the objective was to find assertions that are warranted for any system whatever.

Classification of the newer fields and their relations to each other is difficult. For example, some writers on general systems regard that field as paralleling or even encompassing the other fields discussed in the latter part of this book. (Ludwig von Bertalanffy, "General Systems Theory—A Critical Review," *General Systems*, Vol. VII, 1962; Kenneth Boulding, "General Systems Theory—The Skeleton of Science," *Management Science*, Vol. 2, 1956.) Game and decision theories have many similarities, such as emphasis on rational behavior and maximizing utilities; consequently we have considered both fields in a single chapter. We focus our attention there primarily on the development of formal models, although not all work on games and decisions is formalistic. In jurisprudence, political science, and other areas, for example, many inquiries into decisions do not rely on mathematical models of the type discussed here. Although some work in information theory is far removed from some work in cybernetics, there also are numerous connections. The notion of *quantity of information*, for example, often is emphasized in cybernetics. We therefore also discuss both of those fields in a single chapter.

Other fields or labels could have been selected for discussion in the latter part of this book than those we did select. For example, much work in organization theory is believed to have applications across many of the traditional fields and resembles the work we discuss. To illustrate, one commentary lists the following as supplying the "theoretical underpinnings of organization theory in general": cybernetics (including information theory), topology (including network theory), and decision theory (including game theory). (Anatol Rapoport and William J. Horvath, "Thoughts on Organization Theory," reprinted in Walter Buckley, ed., *Modern Systems Research for the Behavioral Scientist*, Chicago, Aldine, 1968, p. 75.)

The common theme in the fields we discuss is the development of formal models (usually highly mathematical) in which "solutions" can be derived rigorously; models that are believed to be so representative of the problems of men-in-society that the solutions derived from the model apply to those problems. We conclude, however, that the alleged conformances often do not hold and that highly exaggerated claims for the applications have been made.

Considering the prisoner's dilemma game will illustrate our criticisms. In recent years this aspect of game theory has received much attention (see, for example, Anatol Rapoport and Albert M. Chammah, *Prisoner's Dilemma*, Ann Arbor, University of Michigan Press, 1970). Often far-reaching consequences are drawn for the problems of national and international policy, as well as for the guidance of individuals.

Without implying that his discussion is typical of most work on this game, we criticize in some detail the discussion of the influential economist, Paul A. Samuelson (all references are to pp. 482-483 of his *Economics*, 8th ed., New York, McGraw-Hill, 1970). He begins the section entitled "Prisoner's dilemma and love" by saying: "Game theory can throw light on one of the great needs of our age—the need for altruism." He concludes his discussion by saying: "Thus, game theory does point up the need for brotherhood and common rules of the road." He further says that although in one type of economic situation (a perfectly competitive market), "the maximum of well-being" does result from "the motivation of selfishness," such a result "is a lucky accident," which the "logic" of the prisoner's dilemma game proves is "unlikely to be realized in other social situations."

In showing how game theory can lead to such far-reaching results, Samuelson describes a hypothetical situation in which two criminals are apprehended committing a joint crime. The District Attorney interviews each prisoner *separately*, and informs each one that he has enough evidence to insure that both will get a 1-year prison sentence if neither confesses; that if *both* confess, each will get a 5-year sentence; but if one confesses and the other does not, the person confessing will get a 3-month sentence and the other will get a 10-year sentence. Each prisoner has to weigh the possibilities of what the other prisoner will do. If each separately chooses the decision that could lead to the least possible (3-month) sentence, both will confess and therefore each will receive a 5-year sentence. Their joint self-interest will be served best if neither confesses; both will get a 1-year term (worse than the possible 3-month sentence, but the best "common state"; e.g., sentences totalling *2 years*, compared to sentences totalling *10 years* and *10 years, 3 months*).

Samuelson constructs a "payoff matrix" showing the "utilities" as assumed, and says the following:

> "Note that selfishness leads inevitably to long prison terms—5 years. . . . Only by altruism—or

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*In the first edition of this book, separate chapters were devoted to Game Theory, Decision Theory, Linguistics, Information Theory, Cybernetics, Sign-Behavior, Value Inquiry, and General Systems Theory. For reasons given above, Game and Decision Theory have been combined in one chapter, as have Information Theory and Cybernetics. The chapter on Linguistics has been moved to the earlier part of the book. An extended discussion of sign-behavior in Chapter I and the Appendix (as well as briefer comments in other chapters) replaces the separate chapter on that topic. So much of the chapter on Value Inquiry in the first edition was devoted to traditional speculative materials that we have eliminated the chapter, but some work on values is discussed elsewhere in this edition.

> social agreement (in this case, collusion)—can the best *common* state of the world [1-year sentences for each] be realized.
>
> "To see the need for altruism—or failing that, for collective decision making—apply [the payoff matrix] to the air pollution problem. (Replace not-confess by not-pollute, . . . , etc. And assume that when I pollute and you do not desist from polluting, I will be somewhat worse off if I alone decide to desist.) Then the same logic. . .proves that individualistic pursuit of self-interest leads to everyone breathing the same foul and polluted air that shortens life expectancies."

For this "logic" to "prove" the conclusion, the payoffs must be ordered as in the original example, and they must be known. That many actual social situations are characterized by such ordered and known payoffs seems doubtful; the set of circumstances assumed in the original example does not appear to be typical of the alternatives District Attorneys actually offer to prisoners, and neglects the role of judges and juries in sentencing. Moreover, the "best *common* state of the world" is common *only* to the two prisoners, not to others who may be involved and who may have different "utilities" (such as the victims of the crime, and perhaps the District Attorney, who may regard longer sentences as preferable). The motivation ascribed to the prisoners seems as selfish as the motives Samuelson discussed earlier in connection with a perfectly competitive market; the difference is not between altruism and selfishness, but between different ways of achieving selfish goals (i.e., independently or through collusion). Indeed, to derive "brotherhood," "altruism," "the best common state," etc., from circumstances in which two criminals collude so as to get the shortest sentences they can reflects much the same general procedure as the economic views Samuelson criticized as inapplicable to many social situations; i.e., an optimal social state is derived from the pursuit of self-interest.

Moreover, for the "logic" to apply to air pollution, we should have to know that the payoffs are ordered in the same way they were for the prisoners, as is indicated by the second sentence within parentheses in the long quotation from Samuelson. To say that such a payoff ordering exists seems highly dogmatic; much of the controversy about air pollution legislation concerns disagreements about the relative importance of some particular level of air cleanliness and other considerations.

More generally, whatever force Samuelson's example has in large part stems from a procedure in which aspects or phases of a transaction are reified as separate and interacting "reals"; then the "logic" leads "inevitably" to the conclusion. However, when the transactions are described as they occur, the conclusion loses its "inevitability" and the "logic" may have no pertinence.

Samuelson's comments on game theory and the prisoner's dilemma are more careless than those of many defenders of this type of model, and therefore giving as much space as we have to Samuelson may seem unwise. However, the ease with which such a prominent economist can slide uncritically from conclusions that hold within a model (that itself is unlike actual situations) to far-reaching conclusions about "the human condition" is, we believe, worth pointing out. Moreover, what Samuelson does here unguardedly is done in a more complex and obscure manner by others.

Some readers may feel that the criticisms offered in the following chapters on the newer behavioral areas are unduly harsh and that encouragement rather than negative criticism is appropriate. However, so many grandiose claims have been made for the achievements in these areas, including comparisons to Einstein's general theory of relativity and to Newton's work, that outspoken criticism seems required. The intense desire to accelerate progress in the behavioral sciences, to further interdisciplinary cooperation, and to unify various areas of inquiry, apparently leads many to overestimate seriously what has been accomplished.

X.

GAME AND DECISION THEORY*

1. WORKING DESCRIPTION OF THE FIELD

GAME theorists attempt to describe in mathematical symbols the most advantageous strategies for "rational" participants in situations such as many parlor games, economic transactions, political contests, international relation problems, etc., in which the participants either may compete or cooperate. "Rational" designates different things in different game theory contexts; often it is applied as a short name for maximizing one's gains or minimizing one's losses.

Inquirers into decision-making investigate those aspects of human behavior involving choices among alternatives. Attempts are made to develop criteria for measuring the relative importance of the objectives to be achieved and the probable effectiveness of alternative ways of achieving the desired objective.

As in other new fields, terminology is unsettled. Decision theory sometimes is viewed as encompassing game theory; work done under other labels (e.g., linear programming) often is viewed as part of decision theory; and both game and decision theory are sometimes considered as parts of general systems theory.

2. OTHER DESCRIPTIONS OF THE FIELD

According to Anatol Rapoport:

> "Game theory is an attempt to bring within the fold of rigorous deductive method those aspects of human behavior in which conflict and cooperation are conducted in the context of choices among alternatives whose range of outcomes is known to the fullest extent to the participants." ("Critiques of Game Theory," *Behavioral Science*, Vol. 4, 1959, p. 65.)

Ewald Burger says:

> "The theory of games, founded by von Neumann. . ., concerns *games of strategy*. In contrast to pure games of chance, these are games whose outcome does not depend on chance alone, but also on certain decisions which the players must make during the course of play. Typical examples of such games are parlor games in which the *n* participating players have to make decisions in accordance with certain rules. These decisions and, perhaps, also certain random events (such as dealing cards) determine the course of play and, hence, the winnings and losses of the *n* players. Besides parlor games there are numerous other areas where there are problems in which the interests of several participants are in direct conflict. Such conflicts of interest can often be represented schematically by means of games of strategy as defined above." (*Introduction to the Theory of Games*, trans. by John E. Freund, Englewood Cliffs, Prentice-Hall, 1963, p. 1.)

John von Neumann and Oskar Morgenstern stated as their aim:

> ". . .we wish to find the mathematically complete principles which define 'rational behavior' for the participants in a social economy, and to derive from them the general characteristics of that behavior. . . . For economic and social problems the games fulfill—or should fulfill—the same function which various geometrico-mathematical models have successfully performed in the physical sciences. Such models are theoretical constructs with a precise, exhaustive and not too complicated definition; and they must be similar to reality in those respects which are essential in the investigation at hand." (*Theory of Games and Economic Behavior*, 3rd ed., Princeton, Princeton University Press, 1953, pp. 31-32.)

The field of decision theory sometimes is described very broadly. Sidney Schoeffler, for example, says:

> "*General decision theory* is concerned with the solution of the basic problem of how to decide upon the best course of action in any given set of specific circumstances." (*The Failures of Economics*, Cambridge, Harvard University Press, 1955, p. 159.)

According to Irwin Bross:

> "Just before and during World War II a new concept began to emerge—the concept of *Statistical Decision*. Not only was this new concept comprehensive enough to include all that is currently covered in the subject of statistics, but in addition it involved ideas from other subjects such as the theory of games, cost accounting, information theory, logic, economics, and almost anything else you care to name." (*Design for Decision*, New York, Macmillan, 1953, p. 2.)

After stating that purposefulness "exists only if choice is available to the entity involved and if that entity is capable of choice," Russell L. Ackoff says:

> "The basic problem of Decision Theory is the selection and application of a criterion that should be used for selecting a course of action in (what we have here defined as) a purposeful state. Thus Decision Theory concerns itself with measures of efficiency, value, and effectiveness." ("Towards a Behavioral Theory of Communieation," reprinted in Walter Buckley, ed., *Modern Systems Research for the Behavioral Scientist*, Chicago, Aldine, 1968, p. 210, p. 217.)

C. West Churchman describes decision theory as:

> ". . .an attempt to find criteria for selecting 'optimal' decisions among a set of alternative actions—where optimality is based. . .on some

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*As noted on p. 95, for convenience in this Chapter we group together work in the fields of game theory and decision theory. We also include a brief discussion of operations research in Section 3.

measure of the values of various outcomes that may result from selecting each of the actions." ("Science and Decision Making," *Philosophy of Science*, Vol. 23, 1956, p. 247.)

According to Thomas L. Saaty:

> "Decision-making involves the identification of values, objectives, priorities, means, resources, and constraints under conditions of certainty or uncertainty for short- or long-range local or global purposes. Since organizational structure is an important aspect of a decision process, it is also necessary to consider information flow, level and type of participation, and the socioeconomic framework. Decision rules are then introduced and methods (qualitative or quantitative) are developed to apply these rules. It is mostly these methods that bring decision-making close to mathematics by borrowing ideas from it and frequently expanding and enriching these ideas." ("Operations Research: Some Contributions to Mathematics," *Science*, Vol. 178, No. 4065, 1972, p. 1061.)

Sometimes game theory is regarded as a subdivision of decision theory. Anatol Rapoport and William J. Horvath, for example, describe game theory as "that branch of decision theory in which decisions must be made not only in the face of uncertainty but also taking into account the presence of other decision makers, some or all of whose interests may be opposed to those of ego." ("Thoughts on Organization Theory," reprinted in Buckley, *op. cit.*, p. 75.)

3. METHODS AND TYPES OF INQUIRY

Some of the ways in which game and related "strategic competitions" can vary are in the number of participants, the rules of the game, the relation of moves made through choice and "chance," and whether or not the game can be terminated (according to its rules) in a finite number of moves. Mathematical techniques have been devised that give the most advantageous strategies for each participant *if* certain specified conditions prevail, such as the range of permissible actions for the participants, the probabilities with which chance events occur, the information available to each participant, the criteria for the game's termination, etc. Given such definite conditions and rules, solutions to many games have been derived rigorously.

That "rational" participants are assumed is of particular significance. In Section 3 we discuss some of the difficulties related to "rationality," but for the moment the following will suffice to indicate what is involved:

> "Assuming that behavior is correctly described as the maximization of utility, it is quite another question how well a person knows the function, i.e., the numerical utilities, the others are trying to maximize. Game theory assumes he knows them in full. Put another way, each player is assumed to know the preference patterns of the other players. This, and the kindred assumptions about his ability to perceive the game situation, are often subsumed under the phrase 'the theory assumes rational players.' " (R. Duncan Luce and Howard Raiffa, *Games and Decisions*, New York, Wiley, 1957, p. 5.)

In order to illustrate game theory more specifically, a brief description of one type of game will be given. Constant-sum games are those in which the algebraic sum of the gains and losses of the players always equals a fixed number. Given the strategy of his opponent, the positive or negative expected payoff can be calculated mathematically for a "rational" player in a two-person, constant-sum game. The solution of such a game is described as follows:

> "The solution proposed by von Neumann and Morgenstern makes each player choose that strategy for which the minimal gain is at least as high as, and possibly higher than, the minimal gain guaranteed by any alternative strategy. Thus the player is maximizing the minimum pay off, or 'playing the maximin.' (Because of a customary formulation in terms of minimizing maximal loss or risk rather than maximizing minimal gains this principle is usually referred to as the 'minimax' principle.)" (Leonid Hurwicz, "What Has Happened to the Theory of Games," *Papers and Proceedings, American Economic Review*, Vol. XLIII, No. 2, 1953, p. 399.)

Many other types of games have been analyzed, and attempts have been made to extend game theory so that some of the restrictive assumptions (e.g., that the players possess complete knowledge) are no longer required. (See, for example, Nigel Howard, "Some Developments in the Theory and Application of Metagames," *General Systems*, Vol. XV, 1970.) Numerous problem situations in the behavioral science fields have been regarded as analogous to games of strategy (*Game Theory in the Behavioral Sciences*, edited by Ira R. Buchler and Hugo G. Nutini, Pittsburgh, University of Pittsburgh Press, 1969, illustrates some applications), and scientific inquiry in general has been viewed in terms of the investigator playing a game "against nature" (see, for example, J. Milnor, "Games Against Nature," in R.M. Thrall, C.H. Coombs, and R.L. Davis, eds., *Decision Processes*, New York, Wiley, 1954).

Some work on decisions concerns "empirical decision-making"; inquiries in political science, psychology, sociology and other fields have investigated the processes by which decisions are arrived at, the consequences of decisions, the sources of decision in an organization, etc. Many such workers agree with James G. March that decision making is "one of the key focal points for empirical social science." ("An Introduction to the Theory and Measurement of Influence," in Heinz Eulau, S.J. Eldersveld, and M. Janowitz, eds., *Political Behavior*, Glencoe, Free Press, 1956, p. 385.) Other work puts primary emphasis on the development of formal models for decision-making. We emphasize the work of the model builders in this chapter.

Models are built for decisions to be made under various types of general circumstances. Luce and Raiffa differentiate the following four conditions: (1) *certainty*, in which "each action is known to lead invariably to a specific outcome," (2) *risk*, in which "each action leads to one of a set of possible specific outcomes, each outcome occurring with a known probability," (3) *uncertainty*, in which "either action or both has as its consequence a set of possible specific outcomes, but where the probabilities of these outcomes are completely unknown or are not even meaningful," and (4) a combination of *uncertainty* and *risk*. (Luce and Raiffa, *op. cit.*, p. 13.)

When quantitative measures are assigned to the preferences or likings of those involved in the decision and to the probabilities of the possible alternatives, it is possible to calculate which decision will maximize the desired outcome. The mathematical side of decision theory often is technical. Bayesian statistics, for example, has received considerable attention. Without going into the technical work, some notion of what is involved can be gained from the following quotation:

> "Bayesian statistics, a currently controversial viewpoint concerning statistical inference, is based on a definition of probability as a particular measure of the opinions of ideally consistent people. Statistical inference is modification of these opinions in the light of evidence, and Bayes' theorem specifies how such modifications should be made."
>
> "The Bayesian approach is a common sense approach. It is simply a set of techniques for orderly expression and revision of your opinions with due regard for internal consistency among their various aspects and for the data." (Ward Edwards, Harold Lindman, and Leonard J. Savage, "Bayesian Statistical Inference for Psychological Research," *Psychological Review*, Vol. 70, 1963, p. 193, p. 195.)

Some writers concerned with mathematical decision theory are especially interested in the decisions procedures of large groups. Kenneth Boulding, in referring to the development of large organizations, says:

> "This is the movement, which I have called elsewhere the 'organization revolution,' that in the short space of less than a century has led to the rise of such giant organizations as General Motors, the Pentagon, and the Soviet Union. Nobody has yet given the overall movement a name, and it is indeed hard to find a short and vivid one for a movement so extensive and at the same time so subtle and quiet. The only name I can suggest is the *decision systems movement....*" ("Decision-Making in the Modern World," in Lyman Bryson, ed., *An Outline of Man's Knowledge of the Modern World*, New York, McGraw-Hill, 1960, pp. 421-422.)

Some writers believe that there is a need to develop new principles to replace traditional decision-making techniques. For example, Shoeffler says:

> "It is a rather remarkable fact that the principles of decision-making that are in fact employed by most people today are virtually the same as they were 2000 years ago. Thus a United States senator today, in considering the enactment of a new piece of legislation, employs much the same pattern of analysis as his Roman counterpart did in his time for a similar purpose. He also employs the same thought-patterns as the Roman did in deciding what to eat for lunch, in choosing a wife, or in formulating a personal philosophy of life." (Schoeffler, *op. cit.*, pp. 159-160.)

Schoeffler also suggests that the use of statistical decision theory, game theory, welfare economics, operations research, and modern logic can lead to more effective decision making.

Note on Operations Research

Numerous types of inquiry have been given the label "operations research." Robert Dorfman says that "even after a study of hundreds of examples of work classified as operations research, it is by no means clear just what the method is." ("Operations Research," *American Economic Review*, Vol. L, 1960, p. 575.) The English journal, *Operational Research Quarterly*, describes its field vaguely:

> "Operational research may be regarded as a branch of philosophy, as an attitude of mind toward the relations between man and environment and as a body of methods for the solution of problems which arise in that relationship."

However, a good part of the work done under the label "operations research" can be included in what is here called decision theory. For example, Morse and Kimball say: "Operations research is a scientific method of providing executive departments with a quantitative basis for decisions regarding the operations under their control." (P.M. Morse and G.E. Kimball, *Methods of Operations Research*, rev. ed., New York, Wiley, 1951, p. 1.) Kenneth Boulding says the "central aim" of operations research" is the incorporation of quantitative and mathematical techniques into the processing of information relevant to the making of executive decisions, both in government and business." (Boulding, *op. cit.*, p. 438.) And Saaty says: "Operations research is a field of science concerned with developing ideas and methods to improve decision-making." (Saaty, *op. cit.*, p. 1061.)

For a recent discussion of some of the technical work being done in the field, see Leon S. Lasdon, *Optimization Theory for Large Systems* (New York, Macmillan, 1970); for an introductory account, see Hamdy A. Taha, *Operations Research* (New York, Macmillan, 1971).

4. RESULTS ACHIEVED

Perhaps many workers in game and decision theory would agree with Saaty's statement:

> "...we do such research because people have problems and, as scientists, we believe that any model is better than none; it is all right to give bad answers to problems if worse answers would otherwise be given." (Saaty, *op. cit.*, p. 1061.)

However, his statement appears to assume that developing a model—no matter how defective—in a problem area where no model existed previously is equivalent to an improved answer to the problem, which may not be the case; some models may lead to even worse "answers" than we had before.

Considerable differences of opinion can be found concerning the merits of the models developed by game theorists. As Rapoport notes, extremely laudatory claims were sometimes made:

> "In some quarters, game theory was hailed as one of the most outstanding scientific achievements of our century. . . . the implication is that game theory stands on a par with Newton's celestial mechanics as a scientific achievement." (Rapoport, *op. cit.*, p. 49.)

But disillusionment often followed, as Luce and Raiffa note:

> "...we have the historical fact that many social scientists have become disillusioned with game theory. Initially there was a naive bandwagon feeling that game theory solved innumerable problems of sociology and economics, or that, at the least, it made their solution a practical matter of a few years' work. This has not turned out to be the case." (Luce and Raiffa, *op. cit.*, p. 10.)

More recently, one finds few claims that game theory has been an historic breakthrough, but the range of opinion about its merits still is considerable. For example, Saaty believes that game theory may be "one of the most maligned and least appreciated fields of operations research and mathematics." (Saaty, *op. cit.*, p. 1065.) John Boot seems to find much of value in game theory, although he says that "game theory has not quite lived up to the high expectations which were held in the late forties when it was hailed as a major breakthrough in economics." (John C.G. Boot, *Mathematical Reasoning in Economics and Management Science*, Englewood Cliffs, Prentice-Hall, 1967, p. 89.) Others are highly critical of game theory. Walter Berns, for example, is scathing in his analysis of an application to judicial behavior, calling the game theory application "corrupt and untenable," and a substitution of non-sense for common sense. ("Law and Behavioral Science," in Hans W. Baade, ed., *Jurimetrics*, New York, Basic Books, 1963, p. 195.)

Such conflicting assessments appear to stem in large part from differing views concerning the role in inquiry of the models developed by game theorists. Whether the models are descriptive, prescriptive, or something else is discussed in more detail in Section 5 (see also our earlier discussion in Chapter I, pp. 3-4). The earlier workers in game theory tended to argue that their models could be used to describe behavior:

> "...this theory of games of strategy is the proper instrument with which to develop a theory of economic behavior.... We hope to establish satisfactorily...that the typical problems of economic behavior become strictly identical with the mathematical notions of suitable games of strategy." (Von Neumann and Morgenstern, *op. cit.*, pp. 1-2.)

And many recent game theorists also appear to regard their models as descriptively useful. William H. Riker and William J. Zavoina, for example, say that the "message" from their work is that in situations where choice is possible, "utility maximization is the theory that fits political behavior best." ("Rational Behavior in Politics: Evidence from a Three Person Game," *American Political Science Review*, Vol. 64, 1970, p. 60. For further discussion of their work, see Chapter V, p. 50 and p. 56.) In a recent paper, John Fox says that game theory provides "a framework within which to investigate... properties and determinants of social conflict and cooperation," which suggests a descriptive role for game theory. ("The Learning of Strategies in a Simple, Two-Person Zero-Sum Game without Saddlepoint," *Behavioral Science*, Vol. 17, 1972, p. 300.)

Many game theorists, however, maintain that game theory models should not be viewed as descriptive. Anatol Rapoport, for example, has said:

> "I think a categorical disavowal of *descriptive* content is implicit in the entire game-theoretical approach. Game theory is definitely normative in spirit and method. Its goal is a *prescription* of how a rational player should behave in a given game situation when the preferences of this player and of all the other players are given in utility units." (*Fights, Games, and Debates*, Ann Arbor, University of Michigan Press, 1960, pp. 226-227. As we shall see shortly, Rapoport has since modified the views just quoted.)

However, there also are difficulties with the prescriptive interpretation. As Robert L. Davis says:

> "The words 'normative' and 'rational man' seem unfortunate. It may be that the first economists to think in these terms actually intended to set forth what a man *should* do if he were rational; the intention of modern formulators of normative theory is usually much more modest. Each constructs an abstract system about which he says in effect: 'If a person's behavior is such that under the proper interpretation it can be said to satisfy the requirements of this system, then his behavior is what is called "rational" in my theory.' For instance, the theory of zero-sum two-person games can be taken as normative theory in the sense that it gives instructions according to which a man will be able to maximize his expected payoff in such a game, assuming he can find the solution. But this does not say anyone should use this theory in playing an actual game: it may be that he can more easily secure this maximum expected payoff in some other way...." ("Introduction," in Thrall, Coombs, and Davis, *op. cit.*, pp. 4-5.)

Moreover, even when the model provides a description of how a player can maximize expected payoff (and in that sense is normative), many of the problem situations to which game theory has been applied do not conform to the rigorous conditions necessary for the solution to hold. (See Boot, *op. cit.*, pp. 89-90, for a brief discussion of this topic.)

Howard has argued that his "meta-theory" is *both* a theory of rational behavior and an empirical theory that "predicts *actual* behaviour in game-like situations." Without going into the technicalities of his "meta-game," it is important to note that the "actual" behavior predicted is somewhat special: "the theory does not predict that a stable outcome *will* occur, only that if it occurs it will be one of a certain kind singled out by the theory (and called *equilibrium* outcomes)." ("The Theory of Meta-Games," *General Systems*, Vol. XI, 1966, p. 167, p. 168.)

Finally, some writers such as Rapoport argue that game theory models often are neither descriptive nor prescriptive (see Chapter I, p. 3), but still provide a valuable type of *understanding* of certain problems. Rapoport says that game theory imparts "a very special understanding," which is *not* based on prediction, control, or the winning of a competition, but that leads us toward an unspecified and unspecifiable goal:

> "I should like to reiterate my conviction that the understanding of the logical structure of strategic conflicts is indeed the prime and, at least at present, the only achievable objective of game theory.

However, 'understanding' in this context is not the intuitive understanding sought after by the social scientist of the old (pre-positivist) school, nor the understanding of the positivist (rigidly linked to the ability to predict and to control). It is rather the understanding of the mathematician. Based on most rigorous analysis, it is *impersonal* (hence has a partial claim to scientific validity); but it is also independent of the ability to predict or to control (unlike the understanding imparted by sciences with empirical content). The conclusion of a mathematical theorem predicts nothing except that any competent mathematician must come to the same conclusion if he starts from the same hypotheses.

"The mathematician attains 'understanding' by gaining an insight into the interdependence of logical relations. Similarly, a game theoretician attains an 'understanding' of the strategic components of conflict situations by gaining an insight into the often extremely intricate interrelations of strategic considerations. This insight does not reveal techniques for 'controlling' conflicts (let alone means of 'winning' them); but gaining it is an important step forward—toward what we do not know, because we do not know what new goals may be revealed by the increased understanding. Nevertheless it is possible to defend the view that *any* gain of understanding of matters that have some theoretical bearing on important aspects of human relations is a proper human goal." (*N-Person Game Theory*, Ann Arbor, University of Michigan Press, 1969, p. 185, p. 184.)

Rapoport appears to assume here that the mathematical-deductive structures must somehow "bear" on "strategic considerations," which is just the point to be demonstrated. The "logical structure" involved may turn out to be only that of the model, not of the behavior with which Rapoport is concerned. Eliminating from consideration description, prediction, control, and prescriptions for winning may "rescue" game theory from many criticisms, but it also seems to eliminate the pertinence game theory might have for inquiry into observed behavior in "conflict situations."

Despite the many disavowals that game theory is descriptive, inquirers often have no hesitation in viewing some specific "real life" situation as an exemplification of a game. To illustrate, in a recent article the authors give as an example of one of their games a controversy between Orval Faubus and Dale Bumpers in the 1970 Democratic primary in Arkansas. (Marguerite B. Stevenson and James L. Phillips, "Entrapment in 2 x 2 Games with Force Vulnerable Equilibria," *Behavioral Science*, Vol. 17, 1972, p. 369.) Whether in fact such analogies hold is often questionable; similarities of "structure" often appear to be converted into identities of "structure."

In decision theory, considerable "prescriptive" or "normative" work has been done in ascertaining which decisions should be made by a person who has a consistent ordering of preferences and who is striving to maximize some quantity. Such decisions may lead to undesired outcomes, of course, but they are selected because of their *expected* maximization role:

> "If the decision makers use the decision framework of this book, all of their decisions will be 'good' in the sense of maximizing expected utility. Unfortunately, we are still dealing with uncertainty, and a carefully reasoned decision might still have a bad outcome in any particular instance. We do not guarantee good outcomes—just good decisions!" (Albert N. Halter and Gerald W. Dean, *Decisions Under Uncertainty*, Cincinnati, South-Western Publishing Co., 1971, p. 249.)

Experimental work also has been done in comparing observed behavior to what is prescribed by the normative models. For example, Donald Davidson, Patrick Suppes, and Sidney Siegel attempted to provide formal decision models that could be tested empirically. Their "underlying thesis" was that "an individual makes choices among alternatives involving risk as if he were trying to maximize expected utility." (*Decision Making: An Experimental Approach*, Stanford, Stanford University Press, 1957, p. 26.) The following quotation illustrates something of their results:

> "Under controlled conditions, some people (15 out of 19 subjects in the present experiment) make choices among risky alternatives as if they were attempting to maximize expected utility even when they do not make choices in accord with actuarial values." (pp. 80-81.)

In general, observed behavior often seems to depart somewhat from behavior that might be expected on the basis of various decision theories. For example, in an experiment in which subjects gambled on the throw of dice, the following were reported as among the principal results: "expected dollar value has negligible importance in determining betting preferences," and "subjects who are sophisticated about probabilities and expected values are no more likely to maximize expected dollar value than others." (Alvin Scodel, P. Ratoosh, and J.S. Minas, "Some Personality Correlates of Decision Making Under Conditions of Risk," in Dorothy Willner, ed., *Decisions, Values and Groups*, Vol. I, New York, Pergamon Press, 1960, p. 48.)

Many observers construe decision theory as leading to the possible improvement of decisions: "even if a theory of rational decision has little general descriptive value, it still may have great interest as a normative theory." (Davidson, Suppes, and Siegel, *op. cit.*, p. 3.)

Sometimes the assumptions made about human behavior lead to a merging of *normative* and *descriptive*. As Wayne Lee notes:

> "In practice, the distinction between normative and descriptive theories often becomes blurred. After all, the hypothesis of general rationality states than men *do* make the decisions they *should* make. If this is the case, normative and descriptive theories merge into one." (*Decision Theory and Human Behavior*, New York, Wiley, 1971, p. 16.)

Much work in operations research and other areas of decision-making has concerned the improvement of managerial decisions. Russell Ackoff describes a typical inquiry in an industrial setting:

> "In one industrial problem. . .it was necessary to find the order in which items requiring production should be processed over an assembly line. The setup costs associated with each product depended on which item preceded it over the assembly line. The problem was to minimize the sum of the setup costs. . . . Study of the problem revealed several

> decision rules which appeared to yield lower costs than one would expect to obtain by using intuition and experience to sequence the production. . . . The researchers replanned the production of the last three years using the proposed decision rules and compared the resulting costs with those actually incurred. A substantial reduction was obtained." ("Games, Decisions, and Organizations," *General Systems*, Vol. IV, 1959, pp. 149-150.)

Such work has sometimes been criticized, however, on the ground that insufficient evidence is offered for the solutions proposed. Alphonse Chapanis, for example, says:

> "Even when we find model builders attempting to make some validation of their models we sometimes find them using as scientific evidence the crudest form of observations collected under completely uncontrolled conditions. . . . Let us take one real example. Once upon a time the problem of traffic delays at toll booths was tackled [C.W. Churchman, R.L. Ackoff, and E.L. Arnoff, *Introduction to Operations Research*, New York, Wiley, 1957] . . . . They constructed a mathematical model, added it, multiplied it, integrated it, differentiated it, and came out with some conclusions about how the toll booths should be manned and operated. Then came the critical part. Is the model any good? Let us take the authors' own words: 'The only way to find out was to try it. If it worked continuously for a week, it should be able to work indefinitely.' They installed the new system at a toll collecting site and measured traffic flow and some other things for one week. Although the operation of the new system did not conform entirely to expectations there is no doubt that during that week, conditions were better than they had been previously. So say the authors: '. . .there is a good deal of satisfaction in seeing the validity of so much work actually established' . . .Please understand me. The authors may well have been correct. Their system may indeed have been better. But you will have to agree that this kind of test is *not* a model of scientific inference." ("Men, Machines, and Models," *American Psychologist*, Vol. 16, 1961, p. 130.)

5. CONTEMPORARY CONTROVERSY

Although disagreements about the mathematics involved in the elaboration of game and decision theory conjectures do occur, usually such disagreements are satisfactorily resolved; in general there are few controversies about the mathematical soundness of the basic work in the fields. Controversies abound, however, concerning the significance of the models for inquiry into human behavior.

Earlier we discussed disagreements about the role of the models: are they descriptive, prescriptive, or something else? The difficulties with viewing the models as descriptive have led many to emphasize some other role for the models, and yet there is a reluctance to give up completely the notion that observed behavior somehow resembles the model. Rapoport, for example, although insisting that "game-theoretic analysis" is not "a replica of how people actually analyze conflict situations," also suggests that "even though intricate game-theoretic analysis is beyond the conceptual repertoire of the ordinary subject, nevertheless the subjects' intuitions [may] somehow parallel the analysis." (*N-Person Game Theory*, p. 308, p. 310.)

Even if a descriptive role for the models is given up (both officially and in practice), most workers seem to agree that analogies between the models and observed behavior must exist if the models are to be pertinent to the behavior under consideration. Much controversy occurs about such matters. We note first that the presumed connections between the models and observed behavior often are not investigated with anything like the care with which the model is elaborated, and "intuition" may be relied upon heavily. To illustrate, Ewald Burger says:

> "As is the case with any mathematical theory which attempts to describe part of the real world, the fundamental definitions and concepts of the theory of games must be justified by means of intuitive considerations. Unfortunately, this makes it unavoidable to have the rigorous mathematical definitions preceded by certain vague intuitive considerations which establish the connection between the mathematical definitions and reality. This book attempts to reduce these intuitive considerations to a minimum; the author frankly admits that he never feels quite at ease in these intuitive discussions, and because he does not feel competent in this area, the discussion is pushed as rapidly as possible into the domain of mathematics." (Burger, *op. cit.*, p. iii.)

Churchman has been severely critical of those formalists who attempt to justify their models as being in accord with intuition: "whatever proposition has the clear support of intuition needs to be doubted and subjected to analysis." (*Prediction and Optimal Decision*, Englewood Cliffs, Prentice-Hall, 1961, p. 329.)

The total set of conditions required for a mathematically rigorous solution in a game or decision model often is seriously unlike the set of conditions found in the behavioral situations to which the models are applied. For example, the early work on constant-sum games was often applied to economic behavior, but many economic transactions result in a net improvement for all the participants, rather than in winners and losers. Other types of game may be more analogous to such economic transactions, and even within the constant-sum game structure it is "possible to introduce (conceptually) an additional fictitious participant who, by definition, loses what all the real participants gain and *vice versa.*" (Leonid Hurwicz, "The Theory of Economic Behavior," *American Economic Review*, Vol. 35, 1945, p. 918.) But then we are faced again with the question of the analogy between the new model and the behavior under discussion. Although some game theorists are confident that the procedures of "classical" game theory can be extended and modified to overcome the criticisms about applicability, others believe that a much more thoroughgoing reform is necessary. For example, in referring to the book by Luce and Raiffa, Rapoport says:

> "It is primarily their book which convinced me that game theory is more important because of its failures than because of its mathematical successes. For it is the shortcomings of game theory (as originally formulated) which force the consideration of the role of ethics, of the dynamics of social

> structure, and of individual psychology in situations of conflict." (*Fights, Games, and Debates*, p. xii.)

Other controversies concern the notions of utility and rationality as used in the models. In an actual competitive situation, the behavior corresponding to the "utility" in the model may be difficult to identify:

> "A theory such as we are discussing cannot come into existence without assumptions about the individuals with which it purports to be concerned. We have already stated one: each individual strives to maximize his utility. Care must be taken in interpreting this assumption, for a person's utility function may not be identical with some numerical measure given in the game. For example, poker, when it is played for money, is a game with numerical payoffs assigned to each of the outcomes, and one way to play the game is to maximize one's expected money outcome. But there are players who enjoy the thrill of bluffing for its own sake, and they bluff with little or no regard to the expected payoff. Their utility functions cannot be identified with the game payments." (Luce and Raiffa, *op. cit.*, p. 5.)

"Rationality" also is used in diverse ways. For example, Boulding, who is highly sympathetic to decision theory, says the field "is not, it must be confessed, in a state of crystal clarity at the moment, mainly because of the extreme difficulty of deciding on the correct *criteria* of rational decision under conditions of uncertainty." (Boulding, *op. cit.*, p. 438.)

Rapoport points out that what we often call "rational" behavior is not in accord with the assumptions of some writers. For example, we would not necessarily characterize as "rational" the person who based all his decisions on mathematical expectation. Rapoport points out that "the *mathematical expectation* of an individual who takes out fire insurance is clearly negative; otherwise fire insurance companies would go bankrupt." Yet it is not regarded as irrational to purchase fire insurance. "Utility," then, is viewed as something different from mathematical expectation, and "subjective probability" has been introduced for instances in which the individual's estimate of probability differs from the mathematical probability. However, even after such modifications are made, the same problem arises: Is behavior usefully describable using such procedures? Rapoport admits that much doubt is possible:

> "But posing the problem in this way reveals the strong tacit assumption that behavior of individuals or of classes of individuals *is* consistent and predictable, once the underlying utilities and subjective probabilities *exist*. And this may by no means be the case." ("Introduction," in Willner, *op. cit.*, p. xv.)

Moreover, even if in some experimental game or decision setting behavior is found to conform acceptably to the model, the experimental setting itself may be removed considerably from the situation the experiment was designed to describe. In the work by Riker and Zavoina discussed earlier (Chapter V, p. 50 and p. 56), they admit that their experiment dealt with "surrogate politicians in a surrogate political setting." They attempted to remedy disanalogies between their game and actual political situations by introducing "putative equivalents," but then say that "we know, of course, that these putative equivalents are pretty pale imitations of these forces in political life." (Riker and Zavoina, *op. cit.*, p. 59, p. 52.)

Finally, controversy has ensued about the usefulness of formalization. Although most workers rely on elaborate formal models early in their inquiries, C. West Churchman says:

> "Indeed, pure formalization of decision theory seems to be the very last thing we want to do, not the first. For experimental pragmatism—and I suppose equally for operations research—we need to come out of the formal language again, and reach agreements on how observable behavior relates to the terms of the formal language. We need to know when something is a decision—that is, we need operational specifications for identifying decisions and their properties. From this point of view, formal decision theory does not represent a 'foundation' for a theory of decisions." ("Problems of Value Measurement for a Theory of Induction and Decisions," *Proceedings of the Third Berkeley Symposium on Mathematical Statistics*, Berkeley, University of California Press, 1955, p. 55.)

6. TERMINOLOGICAL PROBLEMS

Some of the problems concerning "rationality" and "utility" have already been discussed. "Rationality" is used in different ways in different contexts, and often designates a type of behavior that is not observed in the situations to which the game or decision model is applied. Luce and Raiffa say:

> "Though it is not apparent from some writings, the term 'rational' is far from precise, and it certainly means different things in the different theories that have been developed. Loosely, it seems to include any assumption one makes about the players maximizing something, and any about complete knowledge on the part of the player in a very complex situation, where experience indicates that a human being would be far more restricted in his perceptions." (Luce and Raiffa, *op. cit.*, p. 5.)

The set of requirements necessary for a person to behave rationally may be complicated, as is illustrated by Howard's statement:

> "The rationality axiom is that a person with a choice between outcomes over which he has a preference ordering will choose so as to get the most preferred—*provided* that he fully believes he will get it and that by choosing otherwise he would have got one of the alternatives." ("The Theory of Meta-Games," p. 168.)

Often the use of "rationality" selected is one that will lead to a solution within a model. The following quotation from Rapoport illustrates that situation, as well as the self-actional language often found in this field:

> "Modern decision theory departs from the conception of mechanistic determinants of action and frankly posits a 'rational individual.' Some will insist that 'rationality' is not a well defined category, even that it cannot be defined. But we will forgo the search for a definition which is both general and precise and satisfies every one's intuitive notions of what rationality should be. We will use instead a definition to

> suit a situation. For instance if a man is faced with N mutually exclusive alternatives, we will assume that if he is rational, he is able to arrange the alternatives in order of preference, allowing, perhaps, for indifference among alternatives." ("Introduction," in Willner, *op. cit.*, p. xiv.)

Game and decision theorists often insist that their use of "utility" is not to be confused with the uses of earlier economists. (See pp. 68-69 of Chapter VI for a discussion of economists' use of "utility.") As noted earlier, there is a question as to whether anything in human behavior corresponds to "utility." The characteristics attributed to utility, within a given model, often are chosen in order to provide a logical closure for that model:

> "We have treated the concept of utility in a rather narrow and dogmatic way. We have not only assumed that it is numerical—for which a tolerably good case can be made. . .—but also that it is substitutable and unrestrictedly transferable between the various players. . . . We proceeded in this way for technical reasons: The numerical utilities were needed for the theory of the zero-sum two-person game—particularly because of the role that expectation values had to play in it. The substitutability and transferability were necessary for the theory of the zero-sum *n*-person game. . . . Thus a modification of our concept of utility—in the nature of a generalization—appears desirable, but at the same time it is clear that definite difficulties must be overcome in order to carry out this program." (Von Neumann and Morgenstern, *op. cit.*, p. 604.)

At times highly mentalistic and self-actional language is used in the discussion of utilities. Kenneth Arrow, for example, in his discussion of the impossibility of an "interpersonal comparison of utility," says that "it seems to make no sense to add the utility of one individual, a psychic magnitude in his mind, with the utility of another individual." (*Social Choice and Individual Values*, 2nd ed., New York, Wiley, 1963, p. 11.)

The literature contains frequent references to "applications" of the models, but often that word is used loosely and confusingly. For example, Saaty says that game theory "seeks to be prescriptive or normative," and also—like the later Rapoport—emphasizes the "understanding" that results from the use of game models. He then discusses a "very simple illustration" of a game matrix application to the escalation of arms. (Saaty, *op. cit.*, p. 1066. All further quotations from Saaty in this section are from the same page.)

Saaty says that a few years ago the United States wanted the Soviet Union not to develop MIRV's (multiple independently targeted reentry vehicles). He regards that historical situation as an example of a bargaining game without complete information being available to the players. He then works out a payoff matrix for the situation. According to that matrix, if the United States has MIRV's, and tries to convince the Soviet Union not to develop them, the Soviet Union's payoff will be better if it does develop MIRV's. On the other hand, if the United States lacks MIRV's and tries to convince the Soviet Union not to develop them, the payoff is still better for the Soviet Union if it does produce the weapon.

Saaty concludes that in this situation an opponent "cannot be guided at all" by what the other participant says, and that the opponent's "decision must be based on other information." He suggests that an inspection scheme by both sides might have ensured that MIRV's would not be built, but then adds: "Of course, we know from the newspapers that both sides know how to make MIRV's and may even be mass producing them."

But is the MIRV case an instance of a "bargaining" game in the required sense? Is it probable in this type of situation that either side would be inclined to be guided solely or strongly merely by what the other side said about its state of weapons development? Whatever actual information was available might change the payoff matrix materially. The model is not normative for either side unless the situation fulfills all the basic requirements of the game, which seems highly doubtful; nor is "understanding" of the arms race furthered unless it can be shown that the race fundamentally is like the model. Just what the application of the model is remains unclear.

Other key names in this field also are used unclearly or incoherently. Churchman, for example, points out that often it is not clear what a "decision" is and says that "operational specifications for identifying decisions and their properties should be given." ("Problems of Value Measurement for a Theory of Induction and Decisions," p. 55.)

7. COMMENT AND EVALUATION

In Chapter I we criticized self-actional and interactional procedures of inquiry. Both types of procedure are found among game and decision theorists. Many of the discussions of *rationality* apparently reflect self-actional assumptions, and frequent references to *free will* are found (e.g., Howard, "The Theory of Meta-Games," p. 168; Paul A. Samuelson, *Economics*, 8th ed., New York, McGraw-Hill, 1970, p. 480). Interactional procedures also abound, in which various aspects or phases of a transaction are viewed as independent "reals" interacting on occasion with other such "reals" (see our discussion of the prisoner's dilemma on pp. 95-96).

Much of the work in this field consists in the elaboration of conjectures far in advance of observation. To illustrate, in their discussion of the difficulties of measuring "utility functions," Luce and Raiffa say:

> "The main purpose is to see if under any conditions, however limited, the postulates of the model can be confirmed and, if not, to see how they may be modified to accord better at least with those cases. It will be an act of faith to postulate the general existence of these new constructs, but somehow one feels less cavalier if he knows that there are two or three cases where the postulates have actually been verified." (Luce and Raiffa, *op. cit.*, p. 37. See also our earlier discussion on p. 4, Chapter I.)

As we have noted earlier, in general the mathematical work within game and decision models is carried out rigorously, and much ingenuity has been shown in developing solutions within those models. Moreover, the experimental work on various game and decision situations often is done carefully within the limits of the experiment. But the behavior investigated within these situations may be far removed from the behavior in the economic, political, military, or other transactions that led to the experiment.

Often a major difficulty is the unavailability of what is required to make the model pertinent. To illustrate, James Bates gives the following description of decision-making:

> "A decision-making process involves a decision-maker, an environment in which the decision-maker must operate, a set of actions available, and a set of goals to be accomplished. An optimal decision is made in terms of an operation which chooses a member of the set of actions and the importance of the goals. The model must include some measure of the efficiency of an action for a goal, a measure of the value or importance of the goal, and a measure of the adequacy or effectiveness of the operation." ("A Model for the Science of Decision," *Philosophy of Science*, Vol. 21, 1954, p. 339.)

However, the measures required by the model often don't exist, or are seriously inadequate. Assuming numbers for the sake of illustration may make a model seem plausible and promising, but if we cannot develop those measures in practice the model is not useful for making "optimal decisions."

Our criticisms should not be confused with certain "humanistic" objections to the use of mathematical procedures in investigating human behavior, such as that human behavior involves "intrinsically" unquantifiable things. (For refutations of such objections to mathematical procedures, see von Neumann and Morgenstern, *op. cit.*, pp. 1-8, and Kenneth Arrow, "Mathematical Models in the Social Sciences," *General Systems*, Vol. I, 1956, pp. 29-31.) Skepticism about the significance of a particular mathematical model is not the same as a rejection of mathematical procedures in general.

Defenders of the type of models we have criticized often argue that the historical success of mathematical models in the physical sciences somehow justifies current models in the behavioral areas. For example, Eugene F. Elander, an econometrician, says:

> ". . .to say that, at present, we can stop using mathematical models because they are methodologically so poor that they are not useful for prediction is tantamount to giving up an entire avenue of approach before we have traveled very far along it. A similar criticism might have been raised against the first fumbling attempts to formulate the physical sciences mathematically; and such a criticism, if allowed, would have prevented the development of a scientifically oriented civilization with a high level of technology." ("Correspondence on Mathematical Models," *Social Science*, Vol. 37, 1962, p. 249.)

Elander appears to confuse the avoidance of poor models with the total avoidance of models. Criticism of the inadequacies of existing models may help in the development of improved procedures of inquiry, and reliance on inadequate models may worsen problems that the models were designed to solve. What we object to is not the use of mathematical procedures *per se*, but the development of elaborate conjectures *of any type* far in advance of observation.

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REVIEW OF ECONOMICS AND STATISTICS
SIMULATION AND GAMES
SOCIAL SCIENCE RESEARCH
THEORY AND DECISION

XI.

INFORMATION THEORY AND CYBERNETICS*

1. WORKING DESCRIPTION OF THE FIELD

Information theorists inquire into the most effective way of coding, transmitting, and receiving messages in communication systems in order to achieve goals such as a specified speed of transmission, accuracy of transmission, and economic cost. Quantitative measures have been developed for the capacity of communication channels and for the "amount of information" carried by the signals transmitted over these channels; these measures are based upon the number of alternative messages or signals that can be transmitted over a channel in a unit of time. Many attempts have been made to apply the statistical techniques developed by information theorists, and some of their findings, to typical behavioral science problems.

Cyberneticists inquire into the regulative processes of physical, physiological, and behavioral systems, with special emphasis on feedback in machines and in nervous systems. "Information," as used by information theorists, often is a key notion in cybernetic inquiry.

2. OTHER DESCRIPTIONS OF THE FIELD

R.V.L. Hartley, one of the founders of information theory, said his aim was to "set up a quantitative measure whereby the capacities of various systems [in electrical communication] to transmit information may be compared." ("Transmission of Information," *Bell System Technical Journal*, Vol. 7, 1928, p. 535.)

According to a group of writers on information theory:

> "It is basic to information theory that any event is evaluated against the background of the whole class of events that could have happened. Information theory proposes to measure the effect of operations by which a particular selection is made out of a range of possibilities. . . . The measure of selectivity used is a function of the probability of achieving the same result by mere chance. In this sense, information theory is anchored in probability theory." ("Concluding Review," in Henry Quastler, ed., *Information Theory in Psychology*, Glencoe, Free Press, 1955, p. 8.)

Jerome Rothstein says that "Information theory, in its recent formulations, is a form of abstract mathematics dealing with choices from alternatives of an unspecified nature." (*Communication, Organization, and Science*, Indian Hills, Col., Falcon's Wing Press, 1958, p. 12.)

According to Colin Cherry:

> "It is in *telecommunication* that a really hard core of mathematical theory has developed; such theory has been evolved over a considerable number of years, as engineers have sought to define what it is that they communicate over their telephone, telegraph, and radio systems. In such technical systems, the commodity which is bought and sold, called *information capacity*, may be defined strictly on a mathematical basis, without any of the vagueness which arises when human beings or other biological organisms are regarded as 'communication systems.' Nevertheless, human beings usually form part of telephony or telegraphy systems, as 'sources' or 'receivers'; but the formal mathematical theory is of *direct* application only to the technical equipment itself. . . ." (*On Human Communication*, New York, Wiley, 1957, p. 40.)

John F. Young notes that the "basic idea is. . .that information can be treated very much like a physical quantity such as mass or energy." He says that information theory "is concerned with the basic limitations of various methods of communication," and that the "basic principle adopted is that any message which has a high probability of occurrence conveys little information. Most information is conveyed by the least likely messages. Thus information theory is based on probability. . . ." (*Information Theory*, New York, Wiley, 1971, Preface, p. 1, p. 4.)

Norbert Wiener's statement that cybernetics concerns "the entire field of control and communication theory, whether in the machine or in the animal" (*Cybernetics*, New York, Wiley, 1948, p. 19) often is repeated, sometimes with minor changes or additions. Richard Stone, for example, describes cybernetics as the study of "communication and control in self-regulating systems" (*Mathematics in the Social Sciences and Other Essays*, Cambridge, M.I.T. Press, 1966, p. 33), and F.H. George says cybernetics is the "science of control, communication, and artificial intelligence" (*Computers, Science, and Society*, London, Pemberton Books, 1970, p. 14).

Often strong emphasis is placed on the breadth of application. Stafford Beer, for example, says:

> "Cybernetics is the science of communication and control. The applied aspects of this science relate to whatever field of study one cares to name: engineering, or biology, or physics, or sociology. . . . The formal aspects of the science seek a general theory of control, abstracted from the applied fields, and appropriate to them all." (*Cybernetics and Management*, New York, Wiley, 1959, p. 7.)

And Ralph Parkman says:

> "Cybernetics may be described as the study of brainlike processes or equilibrium-seeking processes and in these kinds of terms it is subject to very broad and often disparate interpretations. It overlaps such fields as general systems theory, theory of automata, semantics, information theory, logic and invades important areas of the physical, natural and social sciences." (*The Cybernetic Society*, New York, Pergamon, 1972, p. 205.)

Anatol Rapoport notes the close relation between information theory and cybernetics, and also emphasizes the technical use of "information," a point we return to later:

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*Information theory and cybernetics are fields that are closely related in many respects. As discussed on p. 95, we include both fields in a single chapter. Some writers use the label "communication theory" rather than "information theory."

> "Cybernetics is the science of communication and control. As such, it does not examine transformations of energy. It examines patterns of signals by means of which information is transmitted within a system and from one system to another. Transmission of information is essential in control, and the capacity of a system to exercise control depends on how much information it can process and store. In fact, the concept 'quantity of information' is central in cybernetics. In this context, 'quantity of information' is unrelated to the meaning of the information, its significance, or its truth. Quantity of information is related simply to the number of 'decisions' which must be made in order to reduce the range of possible answers to the question one asks; to put it in another way, to reduce uncertainty." ("Foreword," in Walter Buckley, ed., *Modern Systems Research for the Behavioral Scientist*, Chicago, Aldine, 1968, p. xix.)

3. METHODS AND TYPES OF INQUIRY

The context in which information theory was developed is of interest. Leon Brillouin says:

> "This new theory was initially the result of a very practical and utilitarian discussion of certain basic problems: How is it possible to define the quantity of information contained in a message or telegram to be transmitted? How does one measure the amount of information communicated by a system of telegraphic signals? How does one compare these two quantities and discuss the efficiency of coding devices?" *(Science and Information Theory*, New York, Academic Press, 1956, p. vii.)

And Yehoshua Bar-Hillel says:

> "One of the tasks with which communication engineers are presented is that of devising a mechanism by which a significant sequence of words, a *message*, produced by somebody, the *sender* of the message, is reproduced at some other place, with the shortest practical time lag. . . . The following illustration is typical: *A* writes on a sheet of paper 'I love you' and wishes that *B*, 3000 miles away, should become aware of the full content of this message, with little delay and at a low cost. There will be institutions, in a capitalistic society, which will compete with each other in providing *A*, for a price, with the required service. Those companies which perform these services most satisfactorily, i.e., with an overall better combination of faithfulness, time lag, and cost, will get the job. The executives of these companies will hire engineers and put them to work on improving this overall combination." ("An Examination of Information Theory," *Philosophy of Science*, Vol. 22, 1955, p. 86.)

A communication system often is said to consist of five parts, as differentiated by Claude Shannon and Warren Weaver: (1) the source of the message; (2) a transmitter, "which operates on the message in some way to produce a signal suitable for transmission over the channel"; (3) the channel, or medium used to transmit the signal to the receiver; (4) the receiver, which decodes the original message from the signal transmitted over the channel; and (5) the destination of the message. (*The Mathematical Theory of Communication*, Urbana, University of Illinois Press, 1949, pp. 4-6.)

In some situations receipt of a near-perfect replica of the message sent is important; in others some deterioration is allowable. Sometimes quick transmission is of primary importance, and sometimes not. If sending the signals over a certain channel is costly, expending sizable sums to develop a compact code for the messages may be worthwhile. In short, both economic and technical factors are involved in ascertaining the most effective way of transmitting messages of a given type.

Of considerable importance are the relative frequencies of letters and letter sequences of the language in which the message is written. Very infrequent letters or sequences can be given code-correlates longer than the code-correlates for frequently occurring letters and sequences. In some instances, whole words, phrases, or sentences may be given a brief code-correlate. However, the saving in transmission time needs to be balanced against the cost of encoding and decoding.

Telecommunication engineers have inquired into the problem of how various signals can be converted into other signals (for example, the translation of letters of the alphabet into the dots and dashes of the Morse code, and the further translation of the Morse code signals into electrical variations). Information theorists have worked on both discrete and continuous signals, and on signal sequences of relatively complex organization (e.g., word or sentence sequences).

Concerned with factors such as those just mentioned, information theorists have developed mathematically a measure of the so-called *amount of information* in a message. Unfortunately, their use of "information" has been confused with other uses (as discussed in Section 4); usually information theorists are concerned with a measure of the relative rarity of a certain signal sequence among all possible signal sequences of the same length. For example, the relative frequency of the sequence *t-h-e* among all three-letter sequences in English interests the communications engineer. Thus Cherry says that information is described "solely as the statistical rarity of signals from an observed source." (Cherry, *op. cit.*, p. 226.)

The unit of measurement commonly used is the *bit* (abbreviation of *binary digit*). Ludwig von Bertalanffy says:

> "Take the game of Twenty Questions, where we are supposed to find out an object by having answered questions about it by yes or no. The amount of information conveyed in one answer is a decision between two alternatives, such as animal or non-animal. With two questions, it is possible to decide for one out of four possibilities. . . . With three questions it is a decision out of eight, and so forth. Thus the logarithm at the basis 2 of the possible answers can be used as a measure of information. . . . The information contained in two answered questions is $\log_2 4 = 2$ bits, of three answers, $\log_2 8 = 3$ bits, and so forth." ("General System Theory," *General Systems*, Vol. I, 1956, p. 5.)

Despite general verbal agreement as to what the word "cybernetics" describes, precisely what inquiries cyberneticists make is not always clear. Beer says: "Some people think that cybernetics is another word for automation; some that it concerns experiments with rats; some that it is a branch of mathematics; others that it wants to build a computer capable of running the country." (Beer, *op. cit.*, p. xii.)

Insofar as cybernetics has been claimed to be relevant for behavioral science inquiry, two types of work seem to be most important:

i) Cyberneticists, like general systems theorists, often seek analogies in different areas of inquiry with the hope that the unification of science will be furthered. Wiener describes one such analogy that stemmed from the efforts of W.S. McCulloch and W.H. Pitts to design an apparatus through which the blind could "read." Printed words were converted to sound by a photocell scanning of the printed type. Wiener goes on to say:

> "Dr. McCulloch's device involved a selective reading of the type-imprint for a set of different magnifications. Such a selective reading can be performed automatically as a scanning process. This scanning, to allow a comparison between a figure and a given standard figure of fixed but different size, was a device which I had already suggested. . . . A diagram of the apparatus by which the selective reading was done came to the attention of Dr. von Bonin [an anatomist], who immediately asked, 'Is this a diagram of the fourth layer of the visual cortex of the brain?' " (Wiener, *op. cit.*, pp. 31-32.)

In particular, much attention has been given to analogies between the nervous system and the digital computer. According to George:

> "It should also be noticed that this [the ease with which computer hardware copes with binary states] is one of the reasons why *cybernetics* pays so much attention to the development of computers. It is because it was felt that there is *a distinct resemblance between the two-state switching devices of the digital computer and the two-state neurons that make up the human nervous system.* Neurons need not necessarily be regarded as two-state switches, but what is important is they *can* be so regarded." (George, *op. cit.*, p. 33.)

ii) A more restricted field of inquiry is the investigation of *feedback* in machines and in organisms. Some writers see the major work of cyberneticists as the inquiry into the extent to which biological phenomena can be accounted for in terms of feedback. According to J.O. Wisdom:

> "The basic hypothesis of cybernetics is that the chief mechanism of the central nervous system is one of negative feed-back. The field of study is not, however, restricted to feed-backs of the negative kind. Secondly, cybernetics makes the hypothesis that the negative feed-back mechanism explains 'purposive' and 'adaptive' behaviour." ("The Hypothesis of Cybernetics," *General Systems*, Vol. 1, 1956, p. 112.)

J.A. Litterer emphasizes the maintenance of system stability through feedback:

> "Cybernetics has to do with feedback and control in all kinds of systems. Its purpose is to maintain system stability in the face of change. Cybernetics cannot be studied without considering communication networks, information flow, and some kind of balancing process aimed at preserving the integrity of the system." (*Organizations*, Vol. I, 2nd ed., New York, Wiley, 1969, p. 24.)

Governors and thermostats illustrate negative feedback. When an engine's speed varies beyond a certain range, the governor operates to maintain the speed within that range. Such feedback is called negative because the work done by the feedback mechanism opposes the "direction" of the main system. Positive feedback occurs when the feedback mechanism acts so as to amplify the work of the main system, as when automobile power brakes amplify foot pressure.

Wiener and his collaborators developed some hypotheses or conjectures about human behavior based on their observations of feedback. Negative feedback may lead to oscillation if the operation of the feedback mechanism results in "overshooting" of a magnitude similar to the deviation from the desired range the feedback mechanism is correcting. For example, in correcting a room temperature that is 5° above what is desired, the temperature may go back to 5° below the setting, etc. Wiener conjectured that oscillation should also be found in organisms, if organisms are "controlled" by negative feedback. One of his collaborators suggested that a patient with cerebellar disease exhibits oscillation. Such a patient cannot directly and smoothly raise a glass to his mouth, but rather overshoots, first in one direction and then in another. Wiener and his co-workers regard this parallel as most helpful: "The analogy with the behavior of a machine with undamped feedback is so vivid that we venture to suggest that the main function of the cerebellum is the control of the feed-back nervous mechanisms involved in purposeful motor activity." (Arturo Rosenbleuth, Norbert Wiener, and Julian Bigelow, "Behavior, Purpose, and Teleology," *Philosophy of Science*, Vol. 10, 1943, p. 20.)

Much excitement about cybernetics stems from the fact that although older machines did not offer many parallels to the complex aspects of human behavior, the development of machines that can play games of strategy, detect and correct some of their errors, store information, etc., does offer such parallels.

As suggested by the foregoing, much work in information theory and cybernetics concerns the questions of machine design and functioning. However, as noted by Parkman, many workers in these areas believe that the significance goes far beyond engineering:

> "To many engineers and scientists it [cybernetics] is a mathematical means of developing the theory and design of computers and other 'brainlike' machines; and among them are some who feel that neither cybernetics nor information theory have fulfilled their original promise in attacking specific technical problems. To others it is less a body of facts or equations than a way of thinking to demonstrate the unity existing between disciplines. They see cybernetics pointing to fundamental meanings we already sense, and offering a theoretical justification for describing the limits of–and probing the still-mysterious boundary between–living and non-living systems. This latter point of view is the one favored here." (Parkman, *op. cit.*, p. 215.)

4. RESULTS ACHIEVED

Conflicting assessments of the merits of the work done in information theory and cybernetics are found. As the passage just quoted from Parkman indicates, even the technical engineering achievements are disappointing to some observers. The significance for behavioral science problems, of course, is of greater importance for the present volume.

As in other newer fields, excessive claims often were made initially. Rapoport notes that information theory was "repeatedly hailed as a 'major breakthrough.'" ("The Promise and Pitfalls of Information Theory," reprinted in Buckley, *op. cit.*, p. 137.) Cybernetics frequently was regarded as a revolutionary new development that shortly would transform the behavioral sciences, but as Caxton Foster observed, the initial enthusiasm frequently was followed by disappointment and disillusionment. (Review of W. Ross Ashby's *An Introduction to Cybernetics*, in *Behavioral Science*, Vol. 2, 1957, p. 319.)

The theme of enthusiasm followed by disillusionment can be illustrated more specifically by what happened in the field of machine translation of languages. In the 1950's, many believed that the translation of, say, Russian into English would soon be a routine computer task. But in 1966 a Committee of the National Academy of Sciences–National Research Council reported that no machine translation of a general scientific text was likely in the near future, and that unedited machine translations were poor in quality and sometimes misleading. ("Languages and Machines: Computers in Translation and Linguistics," Pub. No. 1416, Washington, D.C., National Academy of Sciences, 1966.) Victor H. Yngve notes that "those working in the area [mechanical translation] have consistently underestimated the difficulties throughout the brief history of the field." ("MT at M.I.T. 1965," in A.D. Booth, ed., *Machine Translation*, New York, American Elsevier, 1967, p. 453.) According to George, many of the difficulties seem to stem from a neglect of the *behavioral context* of language use. (George, *op. cit.*, p. 30.) Of interest is Booth's description of the change of attitude of Bar-Hillel, who was a pioneer in machine translation:

> "In the initial phases of the work he was fired with enthusiasm for machine translation but later became known as 'the leader of the destructive school against machine translation,' a position which he can properly be said still to hold. After acting as one of the chief instigators of the First International Conference on Machine Translation held at M.I.T. in 1952, Bar-Hillel drifted steadily from a position of enthusiasm to one of profound gloom about the subject, a drift which resulted in his filling the critical position on the subject which he now does." ("Introduction," in Booth, *op. cit.*, p. vii.)

Basic to many attempted behavioral science applications of information theory is the formula stemming from Shannon's work for a measure of the "amount of information" (H) contained in a message:

$$H = -\Sigma p_i \log p_i$$

(The probability of choice of the i^{th} message is designated by p_i; if the logarithm to the base 2 is used, the amount is given in bits.) Shannon used C to designate the capacity of a communication channel. Brockway McMillan states one of Shannon's basic results concerning the problem of the most efficient coding for a channel:

> "...suppose that the engineer is given a source of rate H, and a channel, totally unrelated to the source, of capacity C. Then Shannon shows that, if H is less than C, the engineer can design (i.e., set up mathematical descriptions of) translating devices for each end of the channel, between the given source and the channel and between the channel and recipient, of such a nature that the text from the source can be recovered by the recipient with a probability of error which can, by design, be made as small as desired. On the other hand, if H exceeds C, there will always remain residual errors in the received text no matter how the encoding and decoding is performed" ("Mathematical Aspects of Information Theory," in Brockway McMillan *et. al.*, *Current Trends in Information Theory*, Pittsburgh, University of Pittsburgh Press, 1953, pp. 10-11.)

In general, many problems of matching sources to a channel, of effectively encoding messages, etc., have been adequately solved. Such useful, if limited, results often have been taken as having far-reaching implications for human communication in general, as is illustrated by the following statement:

> "Indeed, it might be said that all interaction between situations and individuals can be regarded as communication and can be studied with the techniques of communication theory. This is the perspective which has been adopted by Norbert Wiener in his *Cybernetics* (1948) and in *The Human Use of Human Beings* (1950)." (John B. Carroll, *The Study of Language*, Cambridge, Harvard University Press, 1959, p. 205.)

But the "communication" this quotation refers to involves questions of the significance of the messages communicated (what alleged facts the messages state, etc.); technical information theory does not deal with such questions. The confusion between the application of the label "information" in information theory and other applications is widespread. Bar-Hillel says:

> "This christening ["information" as a label for signal sequence] turned out to be a continuous source of misunderstandings, the more so since it sounds so plausible that 'when we speak of the capacity of a system to transmit information we imply some sort of quantitative measure of information'.... However, it is psychologically almost impossible not to make the shift from the one sense of information, for which this argument is indeed plausible, i.e. information = signal sequence, to the other sense, information = what is expressed by the signal sequence, for which the argument loses all its persuasiveness.... Therefore, we see over and over again that, in spite of the official disavowal of the interpretation of 'information' as 'what is conveyed by a signal sequence,' 'amount of information,' officially meant to be a measure of the rarity of kinds of transmissions of signal sequences, acquires also, and sometimes predominantly, the connotation of a measure...of the kinds of facts...designated by these signal sequences." (Bar-Hillel, *op. cit.*, p. 94.)

The confusion Bar-Hillel discusses sometimes occurs in a simple, direct way, as in popular accounts of the amount of information contained in a book or the Sunday issue of the *New York Times*. At other times, the confusion is more subtle. The technical application of "information" is recognized, but yet that application is assumed to have implications for the everyday sense of "information."

For example, James G. Miller says:

> "Throughout this presentation *information* (*H*) will be used in the technical sense first suggested by Hartley in 1928. Later it was developed by Shannon in his mathematical theory of communication. It is not the same thing as meaning or quite the same as information as we usually understand it. *Meaning* is the significance of information to a system which processes it. . . . *Information* is a simpler concept: the degrees of freedom that exist in a given situation to choose among signals, symbols, messages, or patterns to be transmitted." ("The Nature of Living Systems," *Behavioral Science*, Vol. 16, 1971, pp. 279-280.)

However, on the next page, he seems to shift to "information" in a more usual sense:

> "Moreover. . .living systems must have. . .specific patterns of information. For example, some species of animals do not develop normally unless they have appropriate information inputs in infancy. As Harlow showed, for instance, monkeys cannot make proper social adjustment unless they interact with other monkeys during a period between the third and sixth months of their lives." (*Ibid*., p. 281.)

Wayne Lee refers to the "obvious importance of information in decision making" and expresses surprise that there hasn't been more work "bridging information theory and decision theory." When he discusses the cost of obtaining information useful for decision making and related topics, he clearly seems to be using "information" in the everyday sense, and yet he also mentions the "technical sense" of information theory and notes that often decision theorists use "amount of information" to refer to the "number of samples observed rather than information in bits." (*Decision Theory and Human Behavior*, New York, Wiley, 1971, pp. 267-268, p. 249, p. 268.)

In the following quotation from John W. Tukey, calculations based on the technical application of "information" are said to "verify" a maxim that rests on a common-sense application of "information":

> "Modern information theory allows us to verify the old maxim that 'a picture is worth a thousand words.' A thousand words of connected English, at 6 characters (5 letters and space) per word, and 1 bit per character. . ., amounts to 6,000 bits of information, equivalent to a choice among 2^{6000} equally probable patterns. A scatter diagram for n points on a 80 by 80 grid. . .would involve one of $80^{2n} = 2^{12.6n}$ alternatives if the points were distinguishable. If, in addition, all patterns were equally likely, 6,000 bits would correspond to about 500 points. With 80x80 = 6400 cells for the 500 or so points, allowance for overlap and indistinguishability of points need only be small. Thus a scatter diagram of six or seven hundred points, if all diagrams are equally likely, is worth, in *formal information content*, a thousand connected English words.' ("Statistical and Quantitative Methodology," in Donald P. Ray, ed., *Trends in Social Science*, New York, Philosophical Library, 1961, pp. 121-122, italics added.)

Some anthropologists have adopted aspects of information theory (or information theory plus cybernetics). Claude Lévi-Strauss maintains that the brain uses a binary code, and he views human society as a communication-exchange machine. Social phenomena are "messages," for which language is a code (*The Savage Mind*, Chicago, University of Chicago Press, 1966). And Edmund Leach argues for a parallel between the brain and the computer; the brain must follow a "program" that is inherited genetically ("Claude Lévi-Strauss: Anthropologist and Philosopher," in Robert A. Manners and David Kaplan, eds., *Theory in Anthropology*, Chicago, Aldine, 1968).

Probably psychology is the behavioral field to which information theory has been most frequently applied as a "breakthrough" technique. However, many applications did not prove useful. According to Lee J. Cronbach:

> "An examination of the studies employing the Shannon. . .formula, however, suggests strongly that it has been accepted whole, with insufficient scrutiny of its internal workings. As with the famous gift horse, psychologists seem to have felt that it would appear ungrateful to become too inquisitive."
>
> "My examination of papers using Shannon's measure leads me to conclude that many applications of the measure must be regarded only as playful. . . . A use of information theory may be taken seriously when the author shows a specific rationale for describing his data by Shannon's measure. The rationale cannot be merely that he is examining a communication process or something that can be compared to one. He must show that this process is like Shannon's in certain basic particulars. . . . If an investigator's process does not conform to these specifications, he can perhaps modify the Shannon formulation. . .but no one seems to do this. He can demonstrate that his conditions are near enough to Shannon's that discrepancies can be ignored–but this is usually not tested. He can continue to use Shannon's measure as a crude approximation to more appropriate functions–this seems less useful than developing the proper rational function to fit the situation." ("On the Non-Rational Application of Information Measures in Psychology," in Quastler, *op. cit.*, p. 14, pp. 24-25.)

Attempted applications in psychology are still found (see, for example, H.B.G. Thomas, "An Information-Theoretic Model for the Serial Position Effect," *Psychological Review*, Vol. 75, 1968, and Donald P. Spence, "The Processing of Meaning in Psychotherapy: Some Links with Psycholinguistics and Information Theory," *Behavioral Science*, Vol. 13, 1968). It seems fair to say, however, that the major recent impact of information theory on behavioral science problems has been via cybernetics.

Much progress has been made in the design of machines to perform complex tasks, often more efficiently than humans can. As we noted earlier, the parallels between aspects of behavior and the functioning of such machines have led to many cybernetic conjectures about behavior. To illustrate, Wisdom mentions the following, among others: (1) that the brain may become "overloaded" in the same way that a telephone exchange can; (2) that the continuous forward walking of cats with a cauterized interpeduncular nucleus can be explained as the failure of a negative feedback mechanism; (3) that the unceasing swimming until death observed in cuttlefish whose higher brain centers have been removed may be similarly accounted for; and (4) that positive feedback may help to account for some cases of renal disease in which renal damage leads to hypertension, the hypertension in turn leads to further renal disease, and so on, until death. (Wisdom, *op. cit.*, pp. 115-117.)

As one might expect, many conjectures that at first sight seem promising are not confirmed by the evidence. To illustrate, Heinz von Foerster made a study of memory in which he used the principles customarily applied to the so-called decay process in physics and chemistry. His conjecture was that forgetting may be the result of the destruction of some of the elementary brain impressions. He assumed that these impressions were caused by the receptors sensing events. Applying decay principles resulted in a forgetting curve that was contrary to the observed data. By introducing other "forgetting coefficients," von Foerster was able to modify his conjecture so that it conformed to experimental evidence on human memory. He also tried to account for hallucinations and the *dejà-vu* phenomenon. ("Quantum Theory of Memory," in *Transactions of the Sixth American Conference on Cybernetics*, New York, Josiah Macy, Jr. Foundation, 1950.) Beer comments on von Foerster's article: "Where this particular piece of work leads is not yet clear." (Beer, *op. cit.*, p. 38.)

More recently, much attention has been given to *artificial intelligence*, the development of machines or computer program simulations that are capable of performing what usually are viewed as uniquely human modes of behavior. Considerable work also has been done on *self-reproducing machines*, with John von Neumann being a pioneer. ("The General and Logical Theory of Automata," in Lloyd A. Jeffries, ed., *Cerebral Mechanisms and Behavior*, New York, Wiley, 1951.) Although von Neumann's machines have not been built, hardware models of simple self-reproducing systems have been constructed. However, there are important differences between the animal and machine progeny; in animals the infants are simpler than their parents and develop to maturity over time; self-reproducing machines yield fully-formed new machines like the original. (For a recent summary of work in these areas and several others, see Parkman, *op. cit.*, Chs. 8 & 9.)

Attempts have been made to apply cybernetics to many complex forms of behavior of men-in-society. For example, the political scientist, Karl W. Deutsch, saw many potential gains from using cybernetic notions ("Toward a Cybernetic Model of Man and Society," in Buckley, *op. cit.*, and *The Nerves of Government*, New York, Free Press, 1966); E.S. Savas applied cybernetics to New York City government ("Cybernetics in City Hall," *Science*, Vol. 168, 1970). Mervyn L. Cadwallader regarded the "breakthroughs" in information theory and cybernetics as of "considerable significance for sociology" ("The Cybernetic Analysis of Change in Complex Social Organizations," reprinted in Litterer, *op. cit.*, Vol. II), and Amitai Etzioni applied cybernetics to the study of total societies (*The Active Society*, New York, Free Press, 1968; see also the condensation of Etzioni's book by Warren Breed, entitled *The Self-Guiding Society*, New York, Free Press, 1971). Richard Stone argued that economists "for years. . .have been talking cybernetics without knowing it" (Stone, *op. cit.*, p. 33), and Arnold Tustin developed a feedback model of some of J.M. Keynes' economic notions (*The Mechanism of Economic Systems*, London, Heinemann, 1953). Many applications in psychology are found; to mention only one, Tamotsu Shibutani regards the cybernetic model of motivation as superior in many respects to other models ("A Cybernetic Approach to Motivation," in Buckley, *op. cit.*).

In all this work the key question, we suggest, is the extent to which the analogies, models, and simulations are useful. Typically the internal aspects of the model are concentrated upon much more than the "matching" of the model to the behavior being studied. In a recent book reviewing a wide variety of simulations, Michael Inbar and Clarice S. Stoll contrast *reliability* and *validity*. They say: "The basic principle for establishing the reliability of a simulation is that *successive runs should give similar results*. . . . Reliability is not of great concern in computer simulations. . . . With only rare exceptions a computer simulation, once it is operating, is unreliable [an unfortunate typographical error]." They observe that "the easiest and least dependable method of validation" is that the simulation "appears reasonable or has face validity." (*Simulation and Gaming in Social Science*, New York, Free Press, 1972, pp. 278-279, p. 281.)

In short, although both information theorists and cyberneticists have developed many conjectures about behavior, the extent to which those conjectures are warranted usually remains to be seen; highly "reliable" models often lack "validity."

5. CONTEMPORARY CONTROVERSY

The major controversies, both within information theory and the application to other fields, concern the notion of "information." There now seems to be general agreement that the technical use of "information" is considerably different from the everyday use, although such an eminent cybernetician as Wiener regarded "amount of information" and "amount of meaning" as synonyms, and asserted that the amount of meaning can be measured (*The Human Use of Human Beings*, Boston, Houghton Mifflin, 1950, p. 7-8).

Within the group distinguishing between the two senses of "information," some emphasize the difference and see no or few connections. Bar-Hillel, for example, says:

> "But it must be perfectly clear that *there is no logical connection whatsoever between these two measures, i.e. the amount of (semantic) information conveyed by a statement and the measure of rarity of kinds of symbol sequences*. . . . The event of transmission of a certain statement and the event expressed by this statement are, in general, entirely different events. . . ." (Bar-Hillel, *op. cit.*, pp. 95-96.)

Others, however, believe that there is some important connection and that a "theory of meaning" can be built on information theory. Warren Weaver, for example, differentiates three aspects of "the general communication problem": Level A, which concerns the accuracy of signal transmission; Level B, which concerns the precision with which the transmitted signals convey the message; and Level C, which concerns the effectiveness with which the message affects conduct. He goes on to say:

> "...the mathematical theory of communication, as developed by Shannon, Wiener, and others... although ostensibly applicable only to Level A problems, actually is helpful and suggestive for the Level B and C problems."
>
> "The concept of information developed in this theory at first seems disappointing and bizarre—disappointing because it has nothing to do with meaning, and bizarre because it deals not with a single message but rather with the statistical character of a whole ensemble of messages.... I think, however, that these should be only temporary reactions; and that one should say, at the end, that this analysis has so penetratingly cleared the air that one is now, perhaps for the first time, ready for a real theory of meaning."
>
> "One has the vague feeling that information and meaning may prove to be something like a pair of canonically conjugate variables in quantum theory, they being subject to some joint restriction that condemns a person to the sacrifice of the one as he insists on having much of the other." (Shannon and Weaver, *op. cit.*, p. 114, p. 116, p. 117.)

Rapoport also has argued somewhat similarly:

> "It is naive to take simply the flux in signals per second to multiply by bits per signals in the communication engineering sense and call the result 'amount of communication' in the sense of transmission of knowledge (labeling everything one does not like 'noise'). If there is such a thing as semantic information, it is based on an entirely different kind of 'repertoire,' which itself may be different for each recipient.... Yet some meaning lurks in the expression 'to acquire information'! We feel, however vaguely, that as a race we have learned certain things as a result of which we can more effectively 'order the universe' to our liking. We have learned to reverse the degradation of energy locally by putting heat to work.... At this point we could be accused of the same promiscuous speculation we have implicitly warned against. We can only plead that such speculation is extremely difficult to avoid." (Rapoport, "The Promise and Pitfalls of Information Theory," pp. 139-140.)

In cybernetics, probably the major controversies concern the usefulness of analogies, simulations, and models. Some cyberneticists enthusiastically project a "cybernetic control" of large-scale institutions on the basis of the further development of models. Beer, for example (see the quotation from him given on p. 5 of Ch. I), starts with a feedback model of some of Keynes' notions, says that it "must be possible" to construct a simulation model of the economy, that it may be feasible to experiment on that model by means of analogue engineering, and concludes: "And, if all this is possible, then the economy becomes a fit subject for cybernetic control rather than guesswork and the vapourings of political theorists." (Beer, *op. cit.*, p. 35.) Beer assumes here that Keynes' highly controversial original work was sound, that the further models are sufficiently close to what they are modeling to be useful, and other dubious "facts." Ashby mentions numerous possible isomorphisms or parallels in otherwise very different systems, praises general systems theorists for "putting-together" what "classic" scientists did not, apparently confuses the everyday and the technical senses of "information," and then concludes that "information theory will eventually play an important and active part in general systems theory." (W. Ross Ashby, "Editorial," *Behavioral Science*, Vol. 18, 1973, p. 6.)

The development of machines that can perform complex functions has stimulated further discussion of ancient questions such as "Can machines think?" and "Is man a machine?" Such discussions often are bogged down in semantic confusions; considerable reliance is placed on guesses about future developments of machines; and frequently technical disagreements are involved. To illustrate the technical disagreements, brief mention may be made of Gödel's Theorem, a topic on which there is a sizable literature. Kurt Gödel showed that in the formalizations of elementary number theory and its extensions, "true" sentences are formulable that, however, cannot be derived formally within the theory.

Some have argued that this theorem refutes or casts serious doubt on any "mechanistic" interpretation of thinking. Ernest Nagel and James R. Newman, for example, argued that Gödel's theorem "does indicate that the structure and power of the human mind are far more complex and subtle than any non-living machine yet envisaged." *(Gödel's Proof,* New York, New York University Press, 1958, pp. 101-102.) Hilary Putnam, in his review of their book, maintained that the statement just quoted is "simply false." (*Philosophy of Science*, Vol. 27, 1960, p. 207.)

Although the development of cybernetic technology is advancing rapidly, very little can be said with assurance about the eventual adequacy of cybernetic models for behavioral description. (The following illustrate contemporary controversies concerning the thinking ability of machines: Sidney Hook, ed., *Dimensions of Mind*, New York, New York University Press, 1960; Alan R. Anderson, ed., *Minds and Machines*, Englewood Cliffs, Prentice-Hall, 1964; A.M. Turing, "Computing Machinery and Intelligence," *Mind*, Vol. 59, 1950; and Charles S. Chihara, "On the Alleged Refutations of Mechanism Using Gödel's Incompleteness Results," *Journal of Philosophy*, Vol. LXIX, 1972.)

6. TERMINOLOGICAL PROBLEMS

The confusions about various applications of the label "information" are sometimes made even worse by attempted clarifications. Often, for example, "information" is distinguished from "meaning," and information theory is said not to be concerned with the latter (e.g., George A. Miller, *Language and Communication*, New York, McGraw-Hill, 1941, p. 41; Shannon and Weaver, *op. cit.*, p. 3, p. 99), but "meaning" is a notoriously misleading name. "Information" in the everyday sense often is taken as equivalent to "knowledge," which also is a confusing name.

"Communication" frequently is used unclearly, and may be discussed in terms of the interactions of "minds." Weaver, for example, says:

> "The word *communication* will be used here...to include all of the procedures by which one mind

> may affect another. This, of course, involves not only written and oral speech, but also music, the pictorial arts, the theatre, the ballet, and in fact all human behavior. In some connections it may be desirable to use a still broader definition. . . ." (Shannon and Weaver, *op. cit.*, p. 95.)

Bar-Hillel regards the confusions in information theory as so great that he suggests the field should be renamed "Theory of Signal Transmission," which would more accurately describe what has been achieved in the field of signal engineering. He also says that a "terminological clean-up is also required for the use (in order to avoid the misuse)" of the following: "sign," "message," "word," "symbol," "signal," "code," "code element," "elementary symbol," "signal sequence," "symbol sequence," etc. In view of his careful documentation of confusions, to read the following by a conference group is somewhat surprising: "In this field [information measures], we seem to be in the fortunate position of having cleared up the conceptual difficulties. The difficulties which remain are by no means minor ones, but they are 'merely' technical." ("Concluding Review," in Quastler, *op. cit.*, p. 3.)

As do general systems theorists, cyberneticists have some problems with "system." Beer, for example, says: ". . .anything that consists of parts connected together will be called a system. For instance, a game of snooker is a system, whereas a single snooker ball is not. A car, a pair of scissors, an economy, a language, an ear, and a quadratic equation: all these things are systems." (Beer, *op. cit.*, p. 9.) But surely the snooker ball "consists of parts connected together" and therefore is a system. And indeed Beer on the very next page says that a single blade of a scissors "contains a system—this time of atomic characteristics." He further says: "So the problem of stating the system we wish to study is by no means easy." (*Ibid.*, p. 10.)

"Control" also is troublesome. Beer says: "This word is not used in the way in which either an office manager or a gambler might use it; it is used as a name for *connectiveness.*" (*Ibid.*, p. 9.) If so, it might be clearer to use "connectiveness" rather than "control." Some of the confusions that result can be illustrated by questions such as "Will machines ever control man?" Wiener has written extensively on this question and expressed worry about humans being victimized by machines:

> "It has even been supposed. . .that the dangers mentioned by Samuel Butler that the machines may to some extent control humanity are absurd and empty. Now that the machines are stepping up one or more stages in their functions. . .the difficulties and dangers conceived by Samuel Butler assume a new actuality." ("The Brain and the Machine [*Summary*]," in Hook, *op. cit.*, p. 114.)

Without minimizing possible social problems arising from increasing automation, "control" used in the sense of "coercion" (as a machine forcing us to do something we do not want to do), should be clearly differentiated from "control" in the sense of "connectiveness," or from other relations that cyberneticists include as aspects of control.

Writers on cybernetics use "purpose" frequently. "Purpose" sometimes is used in a way reminiscent of Aristotelian teleology; sometimes to refer to the function of a machine; and sometimes in the sense of goal-seeking behavior, as when a hungry animal seeks food. At times it seems impossible to ascertain just what is intended. Some cyberneticists have tried to eliminate older teleological notions entirely, but assume that doing so involves a restriction to physicochemical procedures. Ashby, for example, says:

> "No teleological explanation for behaviour will be used. It will be assumed throughout that a machine or an animal behaved in a certain way at a certain moment because its physical and chemical nature at that moment allowed it no other action. Never will we use the explanation that the action is performed because it will later be advantageous to the animal. Any such explanation would, of course, involve a circular argument; for our purpose is to explain the origin of behaviour which appears to be teleologically directed." (*Design for a Brain*, 2nd ed., New York, Wiley, 1960, p. 9.)

Ackoff and Emery, on the other hand, view mechanistic and teleological procedures as more complementary than antithetical; they also argue that humans can seek different goals in the same stimulus situation and thus are unlike servo-mechanisms and other goal-seeking systems (Russell L. Ackoff and Fred E. Emery, *On Purposeful Systems*, Chicago, Aldine-Atherton, 1972).

Sometimes "machine" is given a very broad application:

> ". . .a machine has been defined as a device which is capable of detecting something going on outside itself, of changing its own state, of shifting its position to detect something else and of taking some action which alters what is happening outside it. A typewriter can be fitted to this description; so can a starfish, a steam-hammer and a brain." (Beer, *op. cit.*, p. 91.)

There may be adequate reasons for so applying the label "machine," but if so, it is hardly surprising that many parallels then can be found between machines and organisms. (See also George, *op. cit.*, pp. 38-39, for a discussion of the broad application of "machine.")

7. COMMENT AND EVALUATION

Developments in information theory and cybernetics illustrate two dangers in behavioral inquiry: (1) the serious misunderstandings that can result from terminological confusion; (2) the exaggerated claims that often are made for the usefulness of mathematical techniques and models. The understandable desire to further quantitative measurements sometimes apparently leads to the belief that what has been developed rigorously within a model must be useful in inquiry. To illustrate, in a recent (1971) book, the author first says that although information theory "has not led to any startling new practical inventions" and "has merely confirmed the inventions which came before the theory," there is "one major achievement" that "must not be overlooked":

> "Workers in many different disciplines have found that the theory can be applied to their work. Because of this, the theory has helped in the process of cross-fertilization of ideas from one discipline to another. *It provides a common language, and definable quantities*, for use in many different fields of endeavour." (Young, *op. cit.*, Preface, italics added.)

Many others also were so enthusiastic about the quantitative aspects of the models they apparently failed to note that the "information" measured through the models is not the "information" usually of concern to behavioral scientists; i.e., a "common language" was *not* developed. Our criticism is well expressed by Bar-Hillel: "It is up to the engineers to revise their terminology, not in order to please some overpedantic philosopher or logician but in order to save themselves futile discussions and to discourage others from ill-advised 'applications.'" (Bar-Hillel, *op. cit.*, p. 104.)

As we have noted earlier, although even the merits of the engineering developments in these fields sometimes have been exaggerated, strong claims still are found about the future, both in respect to engineering and human behavioral applications. George, for example, says: "Cybernetics, as it reaches its fulfillment, will almost certainly supply the automation of the thinking processes which will eventually plan and organize our society as a whole." (George, *op. cit.*, p. 90.)

In addition, some workers in these fields claim originality for views that were developed much earlier, and sometimes more coherently. To illustrate, recently William T. Powers has criticized conventional behaviorist psychologists for relying on the notion that stimuli cause responses, but not vice-versa. He suggests a "new theoretical approach to behavior" that will give attention to "feedback effects"; "cause and effect lose their distinctness and one must treat the closed loop as a whole rather than sequentially." ("Feedback: Beyond Behaviorism," *Science*, Vol. 179, 1973.)

We believe that such criticisms of conventional S-R views have been developed earlier and more adequately by Dewey and Bentley. In his well-known 1896 article, Dewey argued that stimulus and response are not immediate data, but truncated partial statements of what happens, and that for adequate description both need to be considered as phases of a common event. ("The Reflex-Arc Concept in Psychology," *Psychological Review*, Vol. 5, 1896.) In later years he developed that point of view more adequately, culminating in the transactional procedures of inquiry developed with Bentley.

More generally, the holistic emphasis often advocated by cyberneticists usually is combined with interactional procedures and assumptions (e.g., see Ashby's "Editorial"), which leads to incoherence. Trying to "put together" in a holistic system parts that are assumed to exist as separate "reals" apparently accounts for many of the difficulties cyberneticists find in describing adequately some of their key notions, such as that of "system."

Underlying many of the difficulties concerning "meaning," "communication," and "translation" is the reliance on the three-fold distinction of *syntactics*, *semantics*, and *pragmatics*, or similar trichotomies. Much of the work done on the engineering side is in *syntactics* (relation of signals to each other), which does not yield useful results for the study of ordinary human communication. Rudolf Carnap and Bar-Hillel worked on a "theory of semantic information" (with *semantics* viewed as the relation of signs to their referents), but that work also did not deal with ordinary languages or communication (for a brief account of their work, see Cherry, *op. cit.*, pp. 231-243). Later, attempts were made to extend semantic information theory to actual communicative behavior (e.g., Rulon Wells, "A Measure of Subjective Information," *American Mathematical Society Proceedings of Symposia in Applied Mathematics*, Vol. 12, 1961). The syntactics-semantics-pragmatics split has been criticized severely by Dewey and Bentley:

> "Morris attaches himself to Carnap. His contribution lies in the 'pragmatics' he has added to the earlier 'semantics' and 'syntactics'. . .to yield the three 'irreducibles,' the 'equally legitimates'. . . . Carnap gratefully accepts this offering with qualifications. . . . It enables him to toss all such uncomfortable issues as 'gaining and communicating knowledge' to the garbage bucket of pragmatics, while himself pursuing unhampered his 'logical analysis'. . .in the ivory tower of syntactics and in the straggling mud huts of semantics scattered around its base. Neither Carnap nor Morris seems to be aware—or, if aware, neither of them is bothered by the fact—that pragmatism, in every forward step that has been taken in the central line from Peirce, has concentrated on 'meanings'—in other words, on the very field of semantics from which Carnap and Morris now exclude it. To tear semantics and pragmatics thus apart is to lead from Peirce back towards the medieval." (From Ch. I of *Knowing and the Known*, reprinted in R. Handy and E.C. Harwood, *Useful Procedures of Inquiry*, Great Barrington, Behavioral Research Council, 1973, p. 99.)

We suggest that the transactional procedures developed in *Knowing and the Known* are far more adequate for inquiry into sign-behavior, including "meanings" and "communication."

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9. GERMANE JOURNALS

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BELL SYSTEM TECHNICAL JOURNAL
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BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE
BRITISH JOURNAL OF PSYCHOLOGY
BULLETIN OF MATHEMATICAL BIOPHYSICS
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NATURE
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PSYCHOMETRIKA
SCIENCE
SCIENTIFIC AMERICAN
TRANSACTIONS OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS

XII.

GENERAL SYSTEMS THEORY

1. WORKING DESCRIPTION OF THE FIELD

GENERAL systems inquirers attempt to develop warranted assertions that apply either to all systems, whether physical, physiological, or behavioral, or to many systems. They seek analogies, common principles, and "formal identities" that hold across conventionally differentiated fields of inquiry and that will lead to useful integrating conjectures.

2. OTHER DESCRIPTIONS OF THE FIELD

Ludwig von Bertalanffy, a founder of general systems theory, says:

> "...there exist models, principles, and laws that apply to generalized systems or their subclasses, irrespective of their particular kind, the nature of their component elements, and the relations of 'forces' between them. It seems legitimate to ask for a theory, not of systems of a more or less special kind, but of universal principles applying to systems in general. In this way we come to postulate a new discipline called General System Theory. Its subject matter is the formulation and derivation of those principles which are valid for 'systems' in general." ("General Systems Theory," *General Systems*, Vol. 1, 1956, p. 1.)

According to Kenneth Boulding:

> "General Systems Theory is the skeleton of science in the sense that it aims to provide a framework or structure of systems on which to hang the flesh and blood of particular disciplines and particular subject matters in an orderly and coherent corpus of knowledge." ("General Systems Theory: The Skeleton of Science," *General Systems*, Vol. 1, 1956, p. 17.)

James G. Miller, in discussing that part of general systems theory concerned with behavior, says:

> "General systems behavior is concerned with seven levels of living systems—cell, organ, organism, group, organization, society, and supranational system."
>
> "...general systems theorists...accept the... daring and controversial position that—though every living system and every level is obviously unique—there are important formal identities of large generality across levels." ("Living Systems: The Organization," *Behavioral Science*, Vol. 17, 1972, p. 1, p. 2.)

According to Joseph A. Litterer, general systems theory:

> "...is a new and emerging discipline and, as its title suggests, is a general discipline. Usually, scientific disciplines concern themselves with a particular sector of the empirical world.... However, a discipline like mathematics is concerned with developing knowledge that does not of necessity have any connection with the empirical world.... It gives us a language of science."
>
> "General Systems theories are not as abstract or as general as those of mathematics; however, they are considerably more so than the specific formulations of specialized disciplines."
>
> "...If mathematics can be said to provide a language of science, then General Systems may be viewed as providing a skeleton of science. It is concerned with those generalities of theory that occur in more than one specialized discipline and, in fact, may not be able fully to be developed within the confines of one discipline." ("Introduction," in Joseph A. Litterer, ed., *Organizations: Systems, Control and Adaptation*, Vol. II, 2nd ed., New York, Wiley, 1969, pp. ix-x.)

Anatol Rapoport says:

> " 'General system theory' subsumes an outlook or a methodology rather than a theory in the sense ascribed to this term in science. The salient feature of this outlook is, as its name implies, an emphasis on those aspects of objects or events which derive from general properties of systems rather than from the specific content.... The system-theoretic point of view received its impetus from two sources: first, a realization of the inadequacy of 'mechanism' as a univeral model; second, a tendency to counteract the fractionation of science into mutually isolated specialties." ("Mathematical Aspects of General Systems Analysis," reprinted in Litterer, *op. cit.*, Vol. II, p. 88.)

3. METHODS AND TYPES OF INQUIRY

As the passage just quoted from Rapoport suggests, many general systems inquirers reject "mechanistic" procedures in favor of "holistic" or "organismic" procedures, and focus attention on processes rather than structures. "Open systems" are emphasized, in which growth is characteristic, there is a continuous flow of material into the system, the processes involved are often irreversible, and "self-regulation" helps to restore a disturbed balance. As some of the previous quotations also suggest, considerable attention has been given to "unifying" science, although the appropriateness of using different techniques in different scientific subject matters is recognized. (See Peter Caws, "Science and System: On the Unity and Diversity of Scientific Theory," *General Systems*, Vol. 13, 1968.)

Such themes are similar in some respects to Dewey and Bentley's transactional procedures of inquiry (see Ch. I), and some general systems inquirers have emphasized those similarities. (E.g., Charles A. McClelland, "General Systems and the Social Sciences," *ETC.: A Review of General Semantics*, Vol. XVIII, 1962, p. 450; Walter Buckley, ed., *Modern Systems Research for the Behavioral Scientist*, Chicago, Aldine, 1968, p. 384, p. 497.) And Bentley also emphasized similarities between the Dewey-Bentley views and those of von Bertalanffy. (Arthur F. Bentley, *Inquiry into Inquiries*, Boston, Beacon Press, 1954, pp. 349-351.)

However, much work in general systems theory is unlike certain of Dewey and Bentley's key points. Frequent references are found to "mind"; von Bertalanffy, for example, says:

> "The world is, as Aldous Huxley once put it, like a Neapolitan ice cake where the levels, the physical, the biological, the social and the moral universe, represent the chocolate, strawberry, and vanilla layers. We cannot reduce strawberry to chocolate—the most we can say is that possibly in the last resort, all is vanilla, all mind or spirit." ("General Systems Theory," p. 8.)

(Note also the title of one of his books, *Robots, Men and Minds*, New York, Braziller, 1967; references to the "individual mind" also occur in his *General Systems Theory*, New York, Braziller, 1968.)

Although the emphasis on *systems* often is viewed as similar to what Dewey and Bentley named by *transaction*, many general systems inquirers apparently have not understood the differences between transactional and interactional procedures. Much general systems work not only uses the label *interaction*, but proceeds along the lines Dewey and Bentley referred to by that label. Litterer, for example, says:

> "Both general system theory and modern organization theory study:
>
> 1. the parts (individuals) in aggregates, and the movement of individuals into and out of the system.
> 2. the interaction of individuals with the environment found in the system.
> 3. the interactions among individuals in the system.
> 4. general growth and stability problems of systems." (Litterer, *op. cit.*, Vol. I, p. 25.)

For further examples, mention may be made of Buckley's comparison of Dewey-Bentley's work to "the symbolic interactionist theory of mind, self, and society" (Buckley, *op. cit.*, p. 384), and the recent paper by Bruce H. Mayhew, Jr., Louis N. Gray, and Mary L. Mayhew: "The Behavior of Interaction Systems: Mathematical Models of Structure in Interaction Sequence" (*General Systems*, Vol. 16, 1971).

Finally, unlike Dewey and Bentley, many general systems inquirers believe that it is useful to develop elaborate conjectures far in advance of observation; much energy is directed to the formulation of conjectures that hopefully may be useful later. Three recent papers by James G. Miller contain a large number of hypotheses, intended to be evaluated empirically, which he believes may apply widely both to the various "levels" of subject matter he distinguishes and across those levels: "The Nature of Living Systems" (*Behavioral Science*, Vol. 16, 1971); "Living Systems: The Group" (*Behavioral Science*, Vol. 16, 1971); and "Living Systems: The Organization" (*Behavioral Science*, Vol. 17, 1972).

In general, we find that the work of general systems inquirers much more resembles the work of formal model builders as described in Ch. I than the transactional procedures of Dewey and Bentley. Such model building characterizes both of the general systems methods noted by W. Ross Ashby:

> "One, already well developed in the hands of von Bertalanffy and his co-workers, takes the world as we find it, examines the various systems that occur in it. . .and then draws up statements about the regularities that have been observed to hold. This method is essentially empirical. The second method is to start at the other end. Instead of studying first one system, then a second,. . .it. . .considers the set of 'all conceivable systems' and then reduces the set to a more reasonable size. This is the method I have recently followed." ("General Systems Theory as a New Discipline," *General Systems*, Vol. III, 1958, p. 2.)

Much work in general systems has been done by mathematically-oriented biologists and behavioral scientists, but inquirers with a background in engineering have also contributed. What is called "systems theory" or "systems research" in engineering, although often restricted to a relatively narrow field, is similar in some respects to general systems studies. Alphonse Chapanis, in describing the origins of sytems engineering, notes that the individual components of a system (e.g., the telephone system) may meet high specifications, but that "very often the system as a whole will not work as planned when these components are merely joined together." The "complexity of our modern machine assemblages has created the need for systems design and systems engineering." ("Human Engineering," in C.D. Flagle, W.H. Juggins, and R.H. Roy, eds., *Operations Research and Systems Engineering*, Baltimore, Johns Hopkins Press, 1960, p. 337.) This holistic emphasis also occurs in general systems theory. (For a discussion of engineering applications of general systems theory, see George J. Klir, *An Approach to General Systems Theory*, New York, Van Nostrand Reinhold, 1970.)

General systems inquirers are concerned not only with machine and man-machine systems, but with all systems of all kinds. The articles in the yearbook of the Society for General Systems Research, *General Systems*, amply illustrate the great importance its contributors attach to furthering interdisciplinary studies and the unification of science. The techniques used to reach these goals center on finding analogies, "structural isomorphisms," "formal identities," properties, or laws of wide application in different systems. The following quotation is representative:

> "The concept of a system has been identified in a large number of disciplines; biology, management science, economics, sociology, political science, theology, law, etc., have profited by the apparatus associated with general systems. Various authors. . ., recognizing a common thread in the logic of various fields, have identified that core and christened it 'General Systems Theory.' It is hoped that by investigating properties common to all (appropriately designed) systems, applications can be made in the diverse disciplines." (Spyros Makridakis and E. Roy Weintraub, "On the Synthesis of General Systems," Part I, *General Systems*, Vol. XVI, 1971, p. 43.)

These analogies, etc., vary from mathematical models that apply to some, but not all, aspects of the systems studied, to loosely stated verbal parallels. Several illustrations follow.

J.W.S. Pringle argued that there are important parallels between the process of learning and the process of evolution. ("On the Parallel Between Learning and Evolution," *General Systems*, Vol. I, 1956.) Anatol Rapoport found that similar mathematical models can be derived from the spread of neural impulses, rumors, and epidemics. (*Chicago Behavioral Sciences Publications*, No. 1, "Profits and Problems of Homeostatic Models in the Behavioral Sciences," n.d., pp. 19-23.) W. Ross Ashby says that the banking system works "rather like the liver," and also that the dynamics of bank deposits are isomorphic to the dynamics of the "flow of underground water in Arizona." ("Editorial," *Behavioral Science*, Vol. 18, 1973, p. 2, p. 5.)

John W. Thompson compared meteorology and psychology:

> "In the course of studying the behaviour of depressions in accordance with frontal theory, prediction has been improved by breaking away to some extent from the search for uniform and rigid sequences of events, and instead, seeking explanations for sudden, and at first sight unlikely developments, whenever unexpected changes occur. In the same way the prediction of human behaviour might be helped by the study of unexpected changes and apparent inconsistencies." ("Mental Science, Meteorology, and General System Theory," *General Systems*, Vol. V, 1960, p. 25.)

In a later article he sees further parallels among different fields:

> "With regard to classification, the clouds provide the weather expert with a fertile area for study, and the problems facing the meteorologist here resemble not only those in psychology but those in other sciences, such as botany, concerned with growth and development. . . ."
>
> "To a social scientist, the manner in which the planetary wave energy is propagated rapidly to distant areas where it may have magnified an unforeseen effect is not unlike that of rumor in human affairs, as described by Allport and Postman in psychology [G.W. Allport and L. Postman, *The Psychology of Rumor*, New York, Holt, 1947] and by Dodd in sociology in connection with Project Revere [S.C. Dodd, "Diffusion Is Predictable: Testing Probability Models for Laws of Interaction," *American Sociological Review*, Vol. 20, 1955]." (John W. Thompson, "Similar Problems in Meteorology and Psychology," *General Systems*, Vol. X, 1965, p. 49.)

James G. Miller writes:

> "From the field of botany we find a remarkable study in systems theory in the slime mold, mentioned by Ralph Gerard. . . . Under conditions of adequate water and food supply a colony of this plant is made up of quite independent individuals, each with its own inputs, outputs, equilibratory mechanisms and ability to reproduce. Under more stressful conditions, when the environment is less favorable, however, these individuals flow together to form what is essentially a single multicellular organism with specialization of function or distribution of labor. Some become central cells, others peripheral cells which always flow toward the center, wherever it may be; some cells reproduce, and others cannot—a remarkable model of how humans band together under stress from a common enemy, as did the Londoners, for example, during the fire raids of World War II." (*Chicago Behavioral Sciences Publications*, No. 1, pp. 8-9.)

4. RESULTS ACHIEVED

The results achieved by general systems inquirers are closely related to controversies about the significance of those results; consequently the material considered in this section overlaps with that considered in the next section. We consider first the general objectives of general systems inquirers, and then turn to the more specific results of their work.

In an early publication, von Bertalanffy said:

> ". . .the aims of General System Theory can be indicated as follows: (a) There is a general tendency towards integration in the various sciences, natural and social. (b) Such integration seems to be centered in a general theory of systems. (c) Such theory may be an important means for aiming at exact theory in the non-physical fields of science. (d) Developing unifying principles running 'vertically' through the universes of the individual sciences, this theory brings us nearer to the goal of the unity of science. (e) This can lead to a much-needed integration in scientific education." ("General Systems Theory," p. 2.)

A similar theme has been expressed more recently by Jere W. Clark:

> "The term, general systems analysis. . .might be better understood if it were labeled 'comparative systems analysis' because it concentrates on metaphorical or analogous or isomorphic *similarities* of large varieties of systems—such as engineering, economic, ethical, and biological systems. Its emphasis is on functional or organic synthesis." ("Systems Education," *International Associations*, No. 2, Feb. 1970, p. 97.)

As is indicated by some of the statements we quoted earlier, sometimes the "integration" or "synthesis" is viewed in terms of "principles," "laws," etc., that apply to *all* systems. However, some general systems inquirers doubt that there are any significant "laws" of such wide applicability. Rapoport, for example, says:

> "In a way, general system theory belies its name, for there is hardly anything but trivialities that can be said of all systems. There are no 'general system laws,' such as that every system tends to maintain equilibrium or homeostasis or that every system is goal-directed." (Review of W. Buckley's *Sociology and Modern Systems Theory*, in *General Systems*, Vol. XIV, 1969, p. 195.)

Perhaps most general systems inquirers would agree that, whether or not there are "laws" applicable to *all* systems, important similarities ("laws," "principles," "generalities," "formal identities," "structural isomorphisms," "logical homologies," etc.) do occur in many systems. References can be found, for example, to "a common thread in the logic of various fields" (Makridakis and Weintraub, *op. cit.*, p. 43); to similarities found in many systems that apply to objects outside the conventionally-differentiated fields (Von Bertalanffy, "General System Theory: A Critical Review," *General Systems*, Vol. VII, 1962, p. 9); to what is derivable only from the general properties of systems and not from the specific content of a system (Rapoport, "Mathematical Aspects of General Systems Analysis," p. 88); and to general systems theory as the "skeleton of science" (Boulding, *op. cit.*, p. 17, and Litterer, *op. cit.*, Vol. II, p. x).

The significance of those similarities is a topic of much controversy, and is discussed further in the next section. For present purposes, we note one criticism and the type of defense often given by general systems inquirers. In a strong criticism of general systems theory, Roger C. Buck says:

> ". . .the general usefulness of analogies, especially as suggesting plausible hypotheses for subsequent testing, is explained in most beginning logic books. But surely, one feels, general systems theory is doing, or at least trying to do, something more than this. . . . Now I am convinced that general systems theory is trying to do something more, but just exactly *what* more it is extraordinarily difficult to discover. And the basic reason for the difficulty is that, after drawing our attention to some positive analogy, these theorists in general simply fail to say anything about what the analogy is supposed to prove or suggest, while nevertheless managing to convey the impression that something pretty momentous has been proved or suggested." ("On the Logic of General Behavior Systems Theory," in H. Feigl and M. Scriven, eds., *Minnesota Studies in the Philosophy of Science*, Vol. I, Minneapolis, University of Minnesota Press, 1956, p. 228.)

General systems inquirers often mention Newton's law of gravitation as an "isomorphy" with wide application. Von Bertalanffy, for example, says:

> "The isomorphy we have mentioned is a consequence of the fact that, in certain aspects, corresponding abstractions and conceptual models can be applied to different phenomena. It is only in view of these aspects that system laws will apply. . . . In principle, it is the same situation as when the law of gravitation applies to Newton's apple, the planetary system, and the phenomenon of tide." ("General Systems Theory," p. 2.)

Whether the isomorphisms discovered by general systems inquirers are as useful as the law of gravitation is another matter; many isomorphisms are not useful in inquiry. As May Brodbeck notes:

> "It is all too easy to overestimate the significance of structural isomorphisms. The fact that all or some of the laws of one area have the same form as those of another need not signify anything whatsoever about any connection between the two areas. To be convinced of this, just think of all the different kinds of things which can be ranked and measured. All have the same structure as arithmetical addition and, to this extent, the same structure as each other. Only the isomorphism *with arithmetic* is in itself significant. . . . But this implies no connection among all those things that are isomorphic to arithmetic, any more than there need be any connection among all the different kinds of things which satisfy the same form of empirical linear equation." ("Models, Meaning and Theories," in Dorothy Willner, ed., *Decisions, Values and Groups*, Vol. I, New York, Pergamon Press, 1960, p. 28.)

Although some work in general systems theory concerns analogies in many systems, other work concerns the development of mathematical models for particular systems, usually in the hope that the models will apply elsewhere as well. The models may be testable in principle but go far beyond the presently available data (see the 1971 and 1972 articles by James G. Miller), or the model may be more thoroughly tested. One instance in which the proponent maintains that the requisite testing has been done is Stuart C. Dodd's "System A," which has as its core assertion: If everyone knowing an item tells it to anyone, with equal opportunity for all tellers and hearers in each successive unit period, then that item will spread in a Gompertz S-shaped growth curve. Dodd argues that his System A was tested by controlled experiments on people and that it was repeatedly confirmed by a nearly perfect correlation of hypothesized and observed behaviors. He also believes that System A can be generalized as a law of pair-interaction to any field of science. ("How Random Interacting Organizes a Population," *Synthese*, Vol. XII, 1960.)

Attempted applications of general systems work has been made in a wide variety of fields, including psychology, sociology, political science, economics, international relations, biology, engineering, education, ecology, management science, psychiatry, law, theology, and ethics. Ervin Laszlo also has applied general systems theory to philosophy, including the area of a "scientific theory of mind." (*Introduction to Systems Philosophy*, New York, Gordon and Breach, 1971; *System, Structure, and Experience*, New York, Gordon and Breach, 1970.) The following seems to us a fair summary of the results achieved in much of the field: Models, usually mathematical, are constructed for a particular system and then a search is made for other systems to which the same models apply. The similarity of the models of various systems hopefully leads to a unification of science and may facilitate prediction of the behavior within those systems.

General systems inquirers frequently emphasize that their models apply only to certain aspects of the systems they are investigating, but they appear to discuss less frequently the significance of what is excluded by their models. Such exclusions may be vital, as is noted by the authors of a recent paper on ecological models:

> "In this paper we have been concerned to make two main points. First, the behavior of the models one makes of systems are conditioned by assumptions made about the long-term behavior of their environments. To assume that landscape develops in a preordained course to a stable equilibrium predetermines that vegetation, and hence animal communities, will also do so; moreover, the structure of the model itself *excludes reciprocal action* of animals on the development of vegetation, and of plants on the development of land-forms. Second, the assumptions of the developmental models limit the type of question which can be asked within the framework of the model. This is not to say that the assumptions and the conclusions drawn from them are necessarily wrong, but rather that they *preclude their own falsification*." (William H. Drury and Ian C.T. Nisbet, "Inter-relations between Development Models in Geomorphology, Plant Ecology, and Animal Ecology," *General Systems*, Vol. XVI, 1971, p. 67, italics added.)

5. CONTEMPORARY CONTROVERSY

Probably the major controversy in this field concerns the significance of the analogies found by general systems theorists. With reference to some of Rapoport's work, Buck says:

> "He has developed equations to describe the spread of neural impulses, the spread of rumors, and the spread of epidemics, and has demonstrated great ingenuity in so doing. And he too has found that 'models of all these phenomena look mathematically very much alike.' . . .I do not see, however, that it detracts from his genuine achievements to point out that this similarity of mathematical models is, for all he tells us, a sheer coincidence." (Buck, *op. cit.*, p. 232.)

In discussing the material from James G. Miller concerning the slime mold and human behavior (see p. 118), Buck says:

> "Well, so what? What are we to conclude from all this? That Londoners are a form of slime mold? That myxamoebae are a sort of city dweller? Or, perhaps, that during the battle of London some citizens, due to their new and more specialized activities, became sterile, while others devoted themselves exclusively to reproductive activities? One finds it difficult to believe that these are the conclusions he is expected to draw, but, if not these, what others? And, if no conclusions, why all the fuss, why bother with the analogy at all?" (Buck, *op. cit.*, p. 229.)

In reply, von Bertalanffy says:

> ". . .Buck has simply missed the issue of a general theory of systems. Its aim is not more or less hazy analogies; it is to establish principles applicable to entities not covered in conventional science. Buck's criticism is, in principle, the same as if one would criticize Newton's law because it draws a loose 'analogy' between apples, planets, ebb and tide and many other entities; or if one would declare the theory of probability meaningless because it is concerned with the 'analogy' of games of dice, mortality statistics, molecules in a gas, the distribution of hereditary characteristics, and a host of other phenomena." ("General System Theory: A Critical Review," p. 9.)

Surely even the severest critics of general systems work would agree that systems theorists do not *aim* at developing "hazy" analogies; the question is, rather, the usefulness of the analogies that are found. Furthermore, as was noted in the earlier quotation from May Brodbeck, even clearly stated and close analogies may lack significance. Although von Bertalanffy disapproves of hazy analogies and says that Miller's discussion of the slime mold contains "hazardous comparisons," his own comparison of the world to a Neapolitan ice cake (see p. 117) seems somewhat "hazy." And some recent general systems work is characterized by vague and romantic language:

> "If you leaf through the volumes of *General Systems*, you will find articles with such titles as 'Similar Problems in Meteorology and Psychology' followed by pages of mathematical symbols. What possible connexion, you may ask, can there be between the way the weather works, and the working of the human mind? But systems thinkers are not interested in content, or in concrete images, only in abstract properties and relationships. Systems thinking is a species of new abstract information, a new Word, or Logos. To repeat a phrase I used earlier, Systems Thinking is void-oriented. And the thought may well have occurred to you that, in this sense, the goal of Systems Thinking and of true religion is the same: to see through the faces of death to the chalice of eternal life; to see through the illusory concreteness of this world, concrete only because chaotic and unconnected, to the abstract splendour of the world which is to come." (Ray I. Walter and Norman I. Walter, "The Equivocal Principle in Systems Thinking," *General Systems*, Vol. XVI, 1971, p. 11.)

Although the critics also maintain that general systems inquirers sometimes neglect the important dissimilarities of the systems they are comparing (e.g., Buck, *op. cit.*, pp. 224-226), one can find numerous statements by general systems workers about the dangers of misusing analogies (e.g., von Bertalanffy, "General Systems Theory," p. 2; James G. Miller, "Toward a General Theory for the Behavioral Sciences," *American Psychologist*, Vol. 10, 1955). Perhaps the most characteristic view of general systems theorists about analogies is expressed in an article by Ralph W. Gerard, Clyde Kluckhohn, and Anatol Rapoport. They say: "Analogical thinking is thus in our view not so much a source of answers on the nature of phenomena as a source of challenging questions." They also point out that the language used in cybernetics and in the mathematical theory of communication often is carried uncritically into other contexts by "insufficiently informed enthusiasts." ("Biological and Cultural Evolution: Some Analogies and Explorations," *Behavioral Science*, Vol. I, 1956, pp. 8-9.) However, they are hopeful that the discovery of analogies may lead to fruitful progress as well as stimulate interdisciplinary cooperation. Nevertheless, some critics still think that general systems inquirers overemphasize the usefulness of the analogies that they find.

Recently Rapoport has argued that even defective analogies may be useful if they extend the "conceptual repertoire" of scientists. After discussing some mathematical models in the areas of international relations and economics, he says:

> "It would be rash to draw definitive conclusions about the stability of the economic or international system from the properties of various hypothetical systems offered as models. However, an examination of these purely theoretical consequences cannot fail to be instructive in the sense of enlarging the conceptual repertoire of the theoreticians. Mathematical models bring to our attention aspects of phenomena which might not otherwise have occurred to us." ("Mathematical Aspects of General Systems Analysis," p. 96.)

However, if the "hypothetical system" is unlike the actual system being studied, "the aspects of phenomena" that we probably would have overlooked may be aspects *only* of the model and not applicable to the processes into which we are inquiring. Similarly the enlargement of our "conceptual repertoire" may not be pertinent to the problem at hand.

Inasmuch as general systems inquirers often cite past instances in which the discovery of analogies led to useful results, a brief consideration of some failures may help to illustrate the critics' point of view. The following account of such failures is based on Wesley C. Mitchell's *Business Cycles: The Problem and Its Setting* (New York, National Bureau of Economic Research, 1927, pp. 12-16). The noted economist W. Stanley Jevons became convinced that solar cycles cause business cycles. His study of English business trends from 1721 to 1878 convinced him that the commercial cycle averaged a length of 10.466 years, which corresponded closely with the length of 10.45 years then given the sunspot cycles. Jevons held that decennial business crises depend upon meteorological variations and that the latter "in all probability" depend upon cosmic variations as evidenced by sunspots, auroras, and magnetic perturbations. Translated into recent language, Jevons found (or thought he had found) significant similarities in three different systems.

Although his analogies may strike some readers as bizarre, no objection is made here on that score; many significant similarities, when first pointed out, seem peculiar or inexplicable. The difficulty is that the apparent similarities disappeared when more adequate measurements were carried out. As Mitchell notes, astronomers changed their estimate of the sunspot cycle, and commercial cycles have departed widely from the decennial norm. Others, including Jevons' son, attempted to modify the solar conjecture. Possibly some such conjecture will be developed someday that will withstand the test of adequate measurement, but confidence in such conjectures is misplaced until verification is achieved.

Mitchell mentions also another "theory" based on even more systems. In 1919 Ellsworth Huntington found some surprising similarities. He observed that a high death rate preceded hard times, a low death rate preceded prosperity, and that the death-rate curve, inverted, agreed well with school attendance fluctuations a year later, New York bank clearings of three years later, National Bank deposits and wholesale prices four years later, and immigration five years later. He concluded that business cycles seemed largely dependent on the community's mental attitude; the mental attitude on health; and health on the weather. (Mitchell, *op. cit.*, p. 15.) These parallels sound very much like some of those discussed by general systems theorists. Whether any such parallels are scientifically useful can hardly be settled by the initial attractiveness they may have; hence the critics emphasize testing.

Although frequently general systems research is said to be based on the use of "hard science" methods, some critics believe that metaphysical speculation plays a large part in the formulation of some of the results. Buck argues that there is a "kinship between Miller's organic theories of groups and societies, and the metaphysical theory of the state as advanced by Hegel and Bosanquet," and later says: "I believe that general systems theory is not in fact science at all, but rather naive and speculative philosophy." (Buck, *op. cit.*, p. 224, p. 226.)

6. TERMINOLOGICAL PROBLEMS

A repeated criticism of general systems work is that "system" is used too inclusively to be scientifically useful.

Some general systems inquirers view a system as *any* set of selected "elements." For example, Walter and Walter say:

> "A system is any set of variables that we may choose to abstract from all the variables in the world. All other variables, not chosen for this set, become parameters of the system. A system, therefore, is very like a 'thing,' an object of thought, to which we give existence or 'standing-out-ness' by abstracting it from the ground, the void—much as a sculptor abstracts a statue from a block of marble." (Walter and Walter, *op. cit.*, p. 11.)

And Ashby says:

> "Can one reasonably start by considering the class of 'all conceivable systems?' I suggest one can. The first objection to be met is that the class is ridiculously wide. It includes for instance the 'system' that consists of the three variables: the temperature of this room, its humidity, and the price of dollars in Singapore. Most people will agree that this set of variables, as a 'system,' is not reasonable, though it certainly exists. . .Consideration of many typical examples shows that the scientist is, in fact, highly selective in his choice of systems for study." (Ashby, "General Systems as a New Discipline," p. 2.)

Other inquirers emphasize the connections or interactions among the parts of the system. James G. Miller, for example, says:

> "Systems are bounded regions in space-time, involving energy interchange among their parts, which are associated in functional relationships, and with their environments." ("Toward a General Theory for the Behavioral Sciences," p. 513.)

Litterer emphasizes interrelations

> "Perhaps the most frequently cited characteristic of a system is that it is comprised of a number of interrelated elements. These elements may be objects or things. . . , attributes of these things, . . . or. . .events that occur. . . . By saying these are interrelated we mean that the characteristic of any element, object, attribute, or event is dependent on the other attributes or events or objects that exist. Furthermore, that a change in any one means some adjustment or change in the others." (Litterer, *op. cit.*, Vol. II, p. 4.)

And von Bertalanffy says:

> "It looks at first, as if the definition of systems as 'sets of elements standing in interaction' is so general and vague that not much can be learned from it. This, however, is not true. Systems can, for example, be defined by certain families of differential equations and if, in the usual way of mathematical reasoning, more specified conditions are introduced, many important properties can be found of systems in general and more special cases." ("General System Theory," p. 3.)

Some writers argue that describing a system adequately is not possible. In a frequently cited article, A.D. Hall and R.E. Fagen give the following "terse and vague" account: *"A system is a set of objects together with relationships* *between the objects and between their attributes."* They go on to say that "one would be hard-pressed to supply a definition of system" that is precise and unambiguous, and that this "difficulty arises from the concept we are trying to define; it simply is not amenable to complete and sharp description." ("Definition of System," *General Systems*, Vol. I, 1956, p. 18.)

As we have noted, and as some of the passages just quoted suggest, often general systems theorists use interactional language and procedures. At the same time, they typically emphasize holistic and organismic notions, which leads to incoherence. Although McClelland (*op. cit.*) argued for transactional procedures, and such procedures seem required if the holistic notions are to be developed consistently, more often a sharp dichotomy is assumed between *object* and *environment* (e.g., Hall and Fagen, *op. cit.*, p. 20), and the parts of a system are regarded as separate "reals."

As we also noted earlier, many different names are applied in this field to the similarities or analogies found ("isomorphism," "formal identity," etc.). Often the differences, if any, implied by the various names is not clear, and the similarities themselves seem to be of many different types. We suggest that communication would be facilitated if a different name were applied to each specific type of similarity.

7. COMMENT AND EVALUATION

Some of the early difficulties in general systems work, as described by Russell L. Ackoff, still seem to be with us today. He said:

> "There is already evidence that, in the systems revolution, modest results tend to be excessively generalized and that assumptions once stated—if stated at all—tend to be ignored in defining the realm to which results can be applied. Furthermore, and perhaps more seriously, there is a tendency for more and more research time to be consumed in efforts to answer questions which have no operational significance. That is, a new type of metaphysics, one which is subtly cloaked in mathematics, is arising and drawing scientists into fruitless inquiries." ("Games, Decisions, and Organizations," *General Systems*, Vol. IV, 1959, p. 145.)

He also characterized as a "fundamental misconception" the failure to distinguish between an *exercise* and a *problem*. An exercise that is important within a model, given the assumptions and techniques of that model, may have significance only within those confines; i.e., there may be nothing in human behavior that corresponds to it. (Ackoff, *op. cit.*, p. 145.)

The discovery of unexpected analogies often helps to further inquiry, but the existence of analogies *per se* is not necessarily useful. We have emphasized throughout this book the testing of conjectures by observation. Some model builders, however, are so certain of the usefulness of their models that they adopt a cavalier attitude towards observation. Rapoport, for example, after saying that his method is *not* to derive conclusions from "masses of data," goes on to say:

> "The method is autonomous and theoretical. An investigation starts with more or less plausible *assumptions* related to some basic quantitative relations which *may* underlie social behavior. *Consequences* of these relations are the end products of the investigation. They may or may not be compared with observations. This is also the method of mathematical physics, and this is why I refer to it as social physics." (*Fights, Games, and Debates*, Ann Arbor, University of Michigan Press, 1960, p. xi.)

One reason we emphasize testing relates to the objectives of general systems inquirers. If the goal is to derive "universal principles applying to systems in general," the task of verification obviously is a large one. Even if the goal is to derive principles applying to many, but not all, systems, verification still is laborious. The many, many historical failures to develop useful integrating systems should not be forgotten; over and over "answers" that seemed promising failed when more adequate observations were made. Moreover, the existence of "formal patterns" common to many systems is not enough to insure that inquiry into human behavior will be furthered. Physicists have discovered many warranted assertions that do apply to humans; yet those assertions may have little significance for solving the problems of men-in-society. For example, the law of falling bodies does apply to humans, yet the topics of major concern to psychologists, anthropologists, sociologists, economists, political scientists, etc., do not seem to be illuminated by that law.

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9. GERMANE JOURNALS

BEHAVIORAL SCIENCE
BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE
GENERAL SYSTEMS
MATHEMATICAL SYSTEMS THEORY
PHILOSOPHY OF SCIENCE
SYSTEMS ENGINEERING

See also the journals listed at the end of Chapter X and Chapter XI.

CONCLUSION

In our survey of the various behavioral fields, we frequently compared certain current procedures of inquiry to the procedures we suggested in Ch. I. We do not imply by those comparisons that our views about inquiry are widely accepted today; indeed, conflicting views appear to find more supporters now than when the first edition of this book appeared.

In the period since the first edition was prepared, two trends seem to have become increasingly apparent. On the one hand, in field after field many inquirers have advocated subjective and mentalistic methods. Sometimes the subjectivists are overtly antiscientific, but sometimes they claim to be substituting improved scientific techniques for outmoded or inappropriate techniques. On the other hand, addiction to deductive model-building has become increasingly evident in numerous fields. Many would agree with Fritz Machlup's assertion: "Explanation in the social sciences regularly requires the interpretation of phenomena in terms of idealized motivations of the idealized persons whose idealized actions bring forth the phenomena under investigation." ("Are the Social Sciences Really Inferior?," *Southern Economic Journal*, Vol. XXVII, 1961, p. 176.)

In some respects these two trends appear to be markedly different, if not opposed. The formal model builders emphasize mathematics, deduction, and ratiocination, while the subjectivists emphasize the qualitative, meanings, and inner states. In other respects, however, the trends are similar, and some recent writers, such as Robert G. Fabian, argue that deductive procedures can be applied usefully to the "motivated, value-directed behavior of human beings." Fabian praises the "fruitfulness" of the "deductive pattern of explanation," which he takes as the "basic theoretical method" used by economists and as "a valid approach to the general study of human behavior." He also concludes that the "moral dimension of human decision-making" is an "indispensable aspect" of the procedures used by the deductive economists. ("Human Behavior in Deductive Social Theory: The Example of Economics," *Inquiry*, Vol. 15, 1972, p. 411, p. 431.)

Both trends also seem to be characterized by the quest for certainty. Many important human problems have no scientific solution (at least as yet), and the data required for a thorough test of the relevant conjectures often are not available. Rather than concentrating on getting the necessary data, a quicker method is sought, and what seems plausible is offered as "truth."

The attractiveness of subjective procedures for many recent inquirers is based partly on the failures of behavioristic techniques in inquiry into sign behavior. However useful those techniques may be for physical and physiological subject matters, they have not been successful in solving typical behavioral problems. The identification of scientific inquiry in general with typical behavioristic and interactional techniques has led many subjectivists to doubt the adequacy of scientific procedures for describing characteristic human behavior:

> "It frequently happens that a disparity arises between phenomena that can be observed with scientific rigor and what is truly of interest in human behavior; the ordinary activities of human beings, *broken down into muscular movement, neural activity and the like*, do not necessarily add up to units of interest to the psychologist—human behavior." (*Ibid.*, pp. 413-414, italics added.)

Transactional procedures of the type described in Ch. I, however, do not attempt to describe sign behavior in terms of muscular movement, etc., nor do they try to "add up" the interactions of presumed separate reals.

Although the subjectivists often make sound criticisms of some allegedly scientific procedures, historically the emphasis on subjective procedures (which frequently is found in times of cultural decline and a "failure of nerve") has not yielded useful warranted assertions, and we see no evidence that the recent versions will be any more successful than were older versions. A reviewer of a recent book using the method of social phenomenology says: "Why does phenomenology promise so much and deliver so little?"; a lament that seems applicable to all the varied subjectivisms now so prominent. (Thelma McCormack, review of R.D. Laing's *The Politics of the Family and Other Essays*, in *Contemporary Sociology*, Vol. 2, 1973, p. 23.)

The extensive use of models, especially mathematical models, often is taken as a sign of scientific maturity; numerous defenses of such models in the behavioral fields refer to the quantitative precision resulting from Newtonian models in physics. The impression is sometimes given that Newton's models, which were intended to apply exactly to data, were confirmed almost immediately by a broad range of measurements, thus showing the "empirical" usefulness of deductive models. According to a recent article by Richard S. Westfall, however, in several important instances Newton distorted the available data in order to "confirm" his conjectures:

> "...[Newton's *Principia*] proposed the exact correlation of theory with material event as the ultimate criterion of scientific truth.
>
> "And having proposed exact correlation as the criterion of truth, it took care to see that exact correlation was presented, whether or not it was properly achieved. Not the least part of the *Principia's* persuasiveness was its deliberate pretense to a degree of precision quite beyond its legitimate claim. If the *Principia* established the quantitative pattern of modern science, it equally suggested a less sublime truth—that no one can manipulate the fudge factor quite so effectively as the master mathematician himself." ("Newton and the Fudge Factor," *Science*, Vol. 179, Feb. 1973, pp. 751-752.)

Westfall analyzes in detail three instances: the acceleration of gravity, the velocity of sound, and the precession of the equinoxes. On the velocity of sound, he says:

> "...the deception in this case was patent enough that no one beyond Newton's most devoted followers were taken in. Any number of things were wrong with the demonstration. It calculated a

> velocity of sound in exact agreement with Derham's figure, whereas Derham himself had presented the conclusion merely as the average of a large number of measurements. Newton's assumptions that air contains vapor in the quantity of 10 parts to 1 and that vapor does not participate in the sound vibrations were wholly arbitrary, resting on no empirical foundation whatever. And his use of the 'crassitude' of the air particles to raise the calculated velocity by more than 10 percent was nothing short of deliberate fraud. The adjustment involved the assumption that particles of water are completely solid. In fact, Newton believed that they contain the barest suggestion of solid matter strung out through a vast preponderance of void." (*Ibid.*, p. 753.)

We of course are not suggesting that Newton's conjectures were necessarily unsound because he distorted the available data to make his conjectures seem confirmed. We also believe that the carryover of Newton's absolutist assumptions was a far more serious impediment to later progress in physics than his "fudging" of the data, but for present purposes we are emphasizing the unsupported confidence he had in his models. In the behavioral areas, probably a far greater danger than a misleading selection of data is the inapplicability or inadequacy of a model for the uses to which it is put. Ingenious and internally consistent models abound and are much admired (e.g., see Chs. X and XI), but, unfortunately, they often do not yield warranted assertions about behavior. Control over the model is substituted for control over the events of concern, often with the assumption that some events somewhere must be describable by the model.

Pointing to mathematical models that have proved useful is one thing, but to conclude that any mathematical model probably may aid progress is quite another matter. Somehow the egregious mistakes attributable to an undue reliance on existing conjectures and data, no matter how sound they appear to be, are easily forgotten. Immanuel Kant, for example, said of logic:

> "...since Aristotle it has not had to retrace a single step, unless we choose to consider as improvements the removal of some unnecessary subtleties, or the clearer definition of its matter, both of which refer to the elegance rather than to the solidity of the science. It is remarkable also, that to the present day, it has not been able to make one step in advance, so that, to all appearance, it may be considered as completed and perfect." (*Critique of Pure Reason*, trans. by F. Max Müller, New York, Macmillan, 1902, p. 688.)

And earlier we quoted John Stuart Mill's statement about economic value: "Happily, there is nothing in the laws of Value which remains for the present or any future writer to clear up; the theory of the subject is complete...." (*Principles of Political Economy*, 1848; quoted from Vol. I of the 1884 edition, New York, D. Appleton, pp. 536-537.)

To give a more recent example, for many years the distinguished physicist, Robert A. Millikan, maintained that atomic energy (whether derived from atomic disintegration or atom building) could never be a practicable source of mankind's energy requirements: To illustrate, in 1930 he said he had "disposed of the process of atomic disintegration, and found it completely wanting as a source of available energy, since the radioactive elements are necessarily negligible in quantity"; he concluded that at most such energy "may perhaps be sufficient to keep the corner peanut and popcorn man going, on a few street corners in our larger towns, for a long time to come, but that is all." And the energy available through the building-up of other elements from hydrogen requires such high pressures and temperatures that "there is not even a remote likelihood that man can ever tap this source of energy at all." (*Science and the New Civilization*, New York, Scribner's, 1930, p. 96, pp. 111-112.)*

Within a particular model, the assumptions may seem so unchallengeable, the facts so certain, and the reasoning so sound that the conclusions are inescapable, yet in instance after instance, later on the assumptions are refuted, the facts proved wrong, or an error in the reasoning is detected. But still most writers take for granted that some final "truth," incontestable in principle, is the objective of inquiry.

Some, perhaps, may interpret our emphasis on the constant interweaving of conjectures and observations as a version of the traditional "empiricist" distrust of "theorizing." That would be a fundamental misapprehension of our views; problems of all types normally require the development of conjectures for their solution. Our objection is not to conjectures *per se*, for we regard them as necessary, but to conjectures that are not used to direct further observation, or that are viewed as beyond further testing.

In many fields, writers who lament the poor state of observational data apparently believe that a remedy is developing further ingenious conjectures. Leonard S. Silk, for example, after discussing the difficulties posed for economists by "imprecise" and "disorderly" information, says:

> "Yet the economist has a secret weapon that other people do not have. What he has that other people do not have is: economic theory. This gives him certain habits of thought that enable him to conceptualize problems that he has not seen before or problems that seem always to confront him in a new way.... This mode of thought develops out of what I would call the economist's quasi-Talmudic training—which is long on discussion and debate, with continuous passage from the specific to the general and back again—savagely close in its textual criticism—skeptical about its own or anyone else's results—complicated and wide-ranging in its style of inquiry." ("The Problem of Communication," *American Economic Review*, Vol. LIV, No. 3, 1964, pp. 599-600. Silk also repeated part of these comments both in the Preface of his *Readings in Contemporary Economics*, New York, McGraw-Hill, 1970, and in his *Nixonomics*, New York, Praeger, 1972, p. 160.)

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*Twenty years later, Millikan repeated the same basic arguments, but with qualifications. He qualified his negative conclusion concerning the building-up possibility by saying "*so far as we can now see*"; of the disintegration possibility, he said: "As an *economical*, long-range source of power for the power industry, in my opinion this method is out. I venture this opinion in spite of George Eliot's warning that prophecy is the most gratuitous form of mistake." (*The Autobiography of Robert A. Millikan*, New York, Prentice-Hall, 1950, p. 274, p. 275.) Of considerable interest is another early statement by Millikan: "...the great blunder which the physics of the past has made has consisted in extending its generalizations with undue assurance into fields in which they have not been experimentally tested,—that is, in treating these generalizations as fixed, *universally* applicable principles instead of as essentially working hypotheses." (*Time, Matter, and Values*, Chapel Hill, University of North Carolina Press, 1932, p. vii.) Vital as that point is, forgetting it is a temptation to which inquirers often succumb.

Despite what he says in praise of the critical abilities of economists, on the next page he goes on to say:

> "Many economists are careless in their public utterance; they will invest endless hours of meticulous work in a journal article on some fine point of theory, and then turn around and—off the top of their heads—dictate into a machine or scribble out an argument designed to move the Congress or the general public on some vital matter of state." (*Ibid.*, p. 601.)

We suggest that underlying many recent views about behavioral inquiry is the conviction that somehow the "mind" can know some things with finality. What is needed, instead, are procedures that view human thinking and knowing in evolutionary perspective. Once thinking is taken as biosocial adjustive behavior, rather than as the operation of a "mind" that can apprehend "reality" truly, we have an opportunity of escaping from the epistemological quagmires that have so impeded inquiry. Not the least of the ironies of our cultural history is that the general procedures of inquiry developed long before the rise of scientific inquiry still tend to dominate much allegedly scientific work. Despite the rejection of the older conclusions about scientific subject matter, the procedures that led to those conclusions often still are followed. Dewey and Bentley's attempt to describe procedures of inquiry that have proved useful in developing warranted assertions therefore has the utmost importance, as is indicated by the concluding paragraph of Dewey's *Logic: The Theory of Inquiry:*

> "Theories of knowledge that constitute what are now called epistemologies have arisen because knowledge and obtaining knowledge have not been conceived in terms of the operations by which, in the continuum of experiential inquiry, stable beliefs are progressively obtained and utilized. Because they are not constructed upon the ground of operations and conceived in terms of their actual procedures and consequences, they are necessarily formed in terms of preconceptions derived from various sources, mainly cosmological in ancient and mainly psychological (directly or indirectly) in modern theory. Logic thus loses its autonomy, a fact which signifies more than that a formal theory has been crippled. The loss signifies that logic as the generalized account of the means by which sound beliefs on any subject are attained and tested has parted company with the actual practices by means of which such beliefs are established. Failure to institute a logic based inclusively and exclusively upon the operations of inquiry has enormous cultural consequences. It encourages obscurantism; it promotes acceptance of beliefs formed before methods of inquiry had reached their present estate; and it tends to relegate scientific (that is, competent) methods of inquiry to a specialized technical field. Since scientific methods simply exhibit free intelligence operating in the best manner available at a given time, the cultural waste, confusion and distortion that results from the failure to use these methods, in all fields in connection with all problems, is incalculable. These considerations reinforce the claim of logical theory, as the theory of inquiry, to assume and to hold a position of primary human importance." (*Logic: The Theory of Inquiry*, New York, Holt, Rinehart and Winston, 1938, pp. 534-535.)

APPENDIX

TRIAL NAMES[1]

A. PRELIMINARY COMMENTS

Language problems frequently impede communication in behavioral scientists' discussions of their inquiries and of the methods applied in such inquiries. This report presents a glossary of some important terms in order to diagnose some of the inconsistencies, incoherencies, or other inadequacies of language and to suggest trial names that may prove useful to behavioral scientists.[2] Unfortunately, misunderstandings easily occur, even in the initial stages of discussion; consequently, aspects of the problem will be discussed before the trial names are suggested.

Many attempts have been made to improve naming in the behavioral sciences, and an extensive literature is concerned with definitions. In this report, no detailed attempt is made to compare our procedures with others. We begin with the procedures developed by Dewey and Bentley. In order to avoid misunderstanding, we emphasize that we are not attempting to develop or prescribe any final group of names. As Dewey and Bentley say:

> "The scientific method neither presupposes nor implies any set, rigid, theoretical position. We are too well aware of the futility of efforts to achieve greater dependability of communication and consequent mutual understanding by methods of imposition. In advancing fields of research, inquirers proceed by doing all they can to make clear to themselves and to others the points of view and the hypotheses by means of which their work is carried on." (Page 89; all citations to *Knowing and the Known* are to the pages as numbered in the reprinting of that book in Rollo Handy and E.C. Harwood, *Useful Procedures of Inquiry*.)

They further say of their procedure:

> "It demands that statements be made as descriptions of events in terms of durations in time and areas in space. It excludes assertions of fixity and attempts to impose them. It installs openness and flexibility in the very process of knowing. . . . We wish the tests of openness and flexibility to be applied to our work; any attempts to impose fixity would be a denial—a rupture—of the very method we employ." (*K&K*, p. 89).

Our intention has been to continue the Dewey-Bentley line of advance, if it is an advance, without assuming that it necessarily is the only or even the best way to proceed. If improvement in efficiency of communication results, some progress will have been made. If instead our work impedes communication, it should be superseded by something more useful.

"Trial" is used here then, to indicate that we do not seek to fix permanently, or even standardize for a long time, the terminology suggested. Under some circumstances, standardization of terminology may have little or no scientific use. The standardization of names in alchemy or astrology, for example, would be pointless for scientific purposes (except in the sense that if all astrologers agreed, refutation of their views might be easier). As scientific inquirers proceed, new similiarities and differences will be discovered in the subject matter of inquiry; consequently, a fixed terminology probably would be a barrier to progress.

"Name" is used here in the Dewey-Bentley manner (See *K&K*, pp. 132-133), although we realize that others use that word differently. Names here are *not* regarded as things separate from, and intermediate between, the organism and its environment. Rather the focus is on naming *behavior* on an organism-environmental transaction. Conventionally, a sharp separation has been made between a word and its so-called "meaning," but here we attempt to keep the whole naming process in view. For us, the import of "H_2O" as a scientific name is understood in relation to current scientific practices; "H_2O" is a shorthand label for certain aspects of a subject matter of inquiry, including the relations among those aspects, as observed by scientists. To concentrate on "H_2O" as a set of marks or sounds radically separated from the thing named, as some epistemologists do, is considered an undesirable separation of things that, from the viewpoint of our purpose here, usually are found together. Specifically, separation of the word, its so-called "meaning," and the word user, frequently results in hypostatization and seemingly insoluble problems of the locus and status of "meanings" and of "knowledge."

In the present context naming is the aspect of knowing with which we are concerned. Naming behavior, as Dewey and Bentley say, "selects, discriminates, identifies, locates, orders, arranges, systematizes." (*K&K*, p. 133.)

Naming can be made "firmer," be more consistently useful, without restricting future revisions. For crude everyday purposes, naming a whale a fish may be useful; but to name it a mammal marks an improvement from the viewpoint of scientific usefulness. Revisions as to what "atom" is used to designate or name also have provided improved naming.

Our procedures in preparing this report are transactional. "Transaction" here designates or is a name for the full ongoing process in a field where all aspects and phases of the field as well as the inquirer himself are in common process. A transactional report is differentiated from self-actional reports (in which independent actors, powers, minds, etc., are assumed to function) and from interactional reports (in which presumptively independent things are found in causal interconnection). "Borrower can not borrow without lender to lend, nor lender lend without borrower to borrow, the loan being a transaction that is identifiable only in the wider transaction of the full legal-commercial system in which it is present as occurrence." (*K&K*, p. 130.)

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[1] Reprinted, with revisions, from Alfred de Grazia, Rollo Handy, E.C. Harwood, and Paul Kurtz, eds., ***The Behavioral Sciences: Essays in Honor of George A. Lundberg***, Great Barrington, Mass., Behavioral Research Council, 1968.

[2] This report relies heavily on the work of John Dewey and Arthur Bentley. See especially their *Knowing and the Known*, Boston, Beacon Press, 1949; reprinted in full in Rollo Handy and E.C. Harwood, *Useful Procedures of Inquiry*, Great Barrington, Mass., Behavioral Research Council, 1973; and Sidney Ratner and Jules Altman, eds., *John Dewey and Arthur F. Bentley: A Philosophical Correspondence, 1932-1951*, New Brunswick, N.J., Rutgers University Press, 1964.

This report also makes use of the survey of the behavioral sciences by Rollo Handy and Paul Kurtz, first edition, *A Current Appraisal of the Behavioral Sciences*, Great Barrington, Mass., Behavioral Research Council, 1964.

The work and accomplishments of scientists have been described in many different ways, and no attempt is made here to settle all controversies or to endorse dogmatically any one view. Perhaps most can agree, however, that an important part of the scientist's job is the increasingly more useful description of things, including their connections and relations, that are differentiated in the cosmos.

Some authors attempt to distinguish sharply between "description" and "explanation." "Description" is used here to *include* what many refer to as "explanation," rather than in a way that contrasts a "mere" or "bare" description with a scientific "explanation." Obviously scientists seek to improve the crude descriptions of common sense, but their improved reports on their subject matter (i.e., what some label "explanations") are also descriptions in the broad sense. For example, a stick partially submerged in water appears to be bent, and a crude description may go no further than to so state. But if a more adequate description is given, in terms of light refraction, human processes of perception, human language habits, etc., then we have what is sometimes called an "explanation." The explanation of the bent appearance consists in a full description of the whole transactional process, which enables us to predict what normal human observers will see, given certain circumstances.

"Warranted assertion" is used here rather than "true statement" (or "true proposition"). "Warranted assertion" seems an appropriate name for the outcome of successful scientific inquiry. The term helps to remind us that the assertion involved is warranted by the procedures of inquiry and is subject to modification or rejection by further inquiry. It also helps to exorcise the ghost that scientists have as their business the discovering of final and fixed generalizations.

As inquiry proceeds, modification of naming is to be expected. The differentiation of water from the rest of the cosmos is useful for daily life, but adopting the scientific name "H_2O" marked an improvement in that further prediction and control was facilitated. Perhaps the development of physics and chemistry will some day result in the further alteration of the naming for what in everyday life is called water.

We deny emphatically that there is any kind of intrinsic or necessary relation between the marks and sounds used in naming and what is named. In that sense, naming is wholly conventional; whether "water," "aqua," or "gkim" is used to refer to a certain liquid makes no difference. (This is not to deny, of course, that specific words are part of particular languages, and identifying "water" as a noun in the English language affords many clues as to how the word will be used by English speakers.) On the other hand, some names are much more useful than others. "H_2O", for example, as used in current physical science, is quite different from "water" taken as designating one of the assumed four primordial elements. Although the whole notational system now used for chemical elements and their combinations is in an important sense descriptive, once the system is chosen, naming within it is determined in major respects by the system. "H_2O" as shorthand for water is not capriciously chosen but rather is the outcome of painstaking and carefully controlled inquiry. In general, then, although there is no ultimately right naming, and although all naming is conventional, scientific naming is neither capricious nor arbitrary.

Sometimes those who object that naming is too simple a process to be of much importance in scientific inquiry take a much different view of the naming process than that offered here. If strong emphasis is put on naming in relation to assertions warranted by testing, then some of those objections, at least, seem to be met. To have labels for differentiated aspects of the cosmos that have been thoroughly tested is one thing. To elaborate a terminology that stands either for aspects that have not been usefully differentiated, or for supposed aspects inconsistent with well-established "if–then" statements, is quite another matter. Perhaps both "phlogiston" and "caloric" had considerable merit as names consistently usable for various processes that at one time were assumed to occur in heat phenomena. Their deficiencies, from the present point of view, were precisely that they did not name differentiated aspects of the cosmos as found by scientific inquiry.

When those terms became entrenched in scientific discourse, however, they were not easily evicted; they were part of a semantic vested interest. Much the same almost certainly applies to many behavioral science terms now in wide and frequent use. Sometimes suggested changes in naming are rejected on the ground that new specifications (scientific namings) omit important connotations the term had in ordinary discourse or in earlier science. Here again the importance of testing can hardly be overemphasized. Rejection of "phlogiston" doubtless omitted what was once dear to many people, yet scientific progress apparently benefited from those omissions.

"Specification" is used here to refer to the naming that has been found useful in science. Specification is a different process than some of the processes frequently named "definition." "Definition" has been used to refer to such diverse things that confusion often results. As Dewey and Bentley say:

> "The one word 'definition' is expected to cover acts and products, words and things, accurate descriptions and tentative descriptions, mathematical equivalences and exact formulations, ostensive definitions, sensations and perceptions in logical report, 'ultimates,' and finally even 'indefinables.' No one word, anywhere in careful technical research, should be required to handle so many tasks." (*K&K*, p. 148.)

Broadly speaking, "definition" often is used to apply to almost any procedure for saying what the so-called "meaning" of a term is. Much of the difficulty with "definition" seems to be just its linkage with "meaning." But leaving that problem aside, a considerable variety of procedures have been used in attempts somehow to designate what a term stands for or has been applied to, and many of those procedures are highly dubious from a scientific point of view.

In this report, "specification" is used as a name for scientific naming; i.e., the efficient (especially useful) kind of designation found in modern scientific inquiry.

B. FURTHER DEVELOPMENT OF SOME BASIC NAMES

In striving for agreement on some firm, coherent, and consistent naming, proceeding initially along roughly evolutionary lines may be helpful. "Cosmos" was selected to name the sum total of the things we can see, smell, taste, hear, and feel (often aided by instruments), including connections among those things, so that we can talk about the sum total of things without repeatedly having to describe them in detail. "Cosmos" is applied to the universe as a whole system, including the speaking-naming thing who uses that name. Moreover, "cosmos" is the name for all that is included in man's knowing behavior from the most distant past discussed in scientifically warranted assertions to the probable future insofar as it is known by scientifically warranted predictions.

Next we differentiate among the vast number of things in the cosmos and select the living things; for these we choose the name "organism." Note that selecting for naming does not imply detaching the physical thing from the cosmos. Everything named remains a part of cosmos with innumerable relations to other parts.

Among the organisms, we further differentiate for the purpose of the present discussion and select for naming ourselves, our ancestors, and our progeny; these we name "man."

We then observe the transactions of man with other aspects and phases of cosmos and note the transactions named "eating," "breathing," etc. Among those numerous transactions, we differentiate further and select for naming the transactions typical of man but found infrequently or not at all in other organisms.

This type of behavior involves processes of a kind such that something stands for or is assumed to refer to something else. Such processes we name "sign behavior," or simply "sign." Note that "sign" is not the name of the thing that stands for something else; "sign," as used here, is the name of the transaction as a whole; i.e., "sign" is the short name for "sign process." For example, the word "cup" is not taken as the sign for the vessel we drink coffee from; rather the word, the container, and the word user all are regarded as aspects or phases (sometimes both) of the full situation. Sign process is the type of transaction that distinguishes some behavioral from physiological processes, a knowing behavior transaction from a transaction such as eating, digesting, seeing, etc. (But no absolute or ultimate separation is suggested; sign processes always include physiological processes and may affect those processes, as when the reading of a telegram containing bad news affects respiration.)

Sign process in evolutionary development has progressed through the following still-existing stages:

a. The signaling or perceptive-manipulative stage of sign in transactions such as beckoning, whistling, frowning, etc.

b. The naming stage as used generally in speaking and writing.

c. The symboling stage as used in symbolic logic and mathematics.

Focusing our attention now on the naming stage of sign process, we choose to name it "designating." Designating always is behavior, an organism-environmental transaction typical primarily, if not exclusively, of man in the cosmos. Designating includes:

1. The earliest stage of designating or naming in the evolutionary scale, which we shall name "cueing."

> "By Cue is to be understood the most primitive language-behavior. . . .Cue, as primitive naming, is so close to the situation of its origin that at timesit enters almost as if a signal itself. Face-to-face perceptive situations are characteristic of its type of locus. It may include cry, expletive, or other single-word sentences, or any onomatopoeic utterance; and in fully developed language it may appear as an interjection, exclamation, abbreviated utterance, or other casually practical communicative convenience." (*K&K*, p. 136.)

2. A more advanced level of designating or naming in the evolutionary scale, which we shall name "characterizing." This name applies to the everyday use of words; usage that is reasonably adequate for many practical purposes of life.

3. For the, at present, farthest advanced level of designation we use "specifying." This name applies to the highly developed naming behavior best exhibited in modern scientific inquiry.

For the purpose of economizing words in discourse, we need a general name for the bits and pieces of cosmos differentiated and named. For this general name we choose "fact." Fact is the name for cosmos in course of being known through naming by man (with man included among the aspects of cosmos) in a statement sufficiently developed to exhibit temporal and spatial localizations. Fact includes all namings-named durationally and extensionally spread; it is not limited to what is differentiated and named by any one man at any moment or in his life time.

Frequently we need to discuss a limited range of fact where our attention is focused for the time being. For this we choose the name "situation." This is the blanket name for those facts localized in time and space for our immediate attention.

Within a situation we frequently have occasion to refer to durational changes among facts. For these we choose the name "events."

Finally, in discussing events we usually have occasion to refer to aspects of the fact involved that are least vague or more firmly determined and more accurately specified. For those we choose the name "object." Object is an aspect of the subject matter of inquiry insofar as it has reached an orderly and settled form, at least for the time being.

Further tentative comments on sign process may be helpful. The transition from sign process at the perceptive-manipulative stage (here designated "signaling") to the initial naming stage (designated "cueing") is a change from the simplest attention-getting procedures, by evolutionary stages, to a somewhat more complex sign process that begins to describe things and events. No clear line of demarcation is found. Some perceptive-manipulative signalings and primitive word cues are descriptive as well as simple alerting behavior.

The transition from cueing to characterizing also reflects evolutionary development with increasing complexity of process, including formal grammar, etc. The further transition from characterizing to specifying in the manner of modern science reflects the further evolutionary development of sign process, a still more complicated procedure.

At first thought the stage we have here designated "symboling" may seem to be a marked departure from, or to reflect a break in, the evolutionary development of sign process. However, mathematical symboling, at least as frequently used in scientific inquiry, may be considered shorthand specifying. Each symbol replaces one or more words. A single mathematical equation may replace a long and involved sentence, even a paragraph, or a longer description in words.

Sometimes symboling is considered to be different from naming, and even Dewey and Bentley speak of it as an "advance of sign beyond naming, accompanied by disappearance of specific reference such as naming develops." (*K&K*, p. 178.) Mathematical inquiry seems in some respects to differ in kind from the designation used in empirical inquiry, yet the mathematical symbols used in scientific inquiry designate something quite specific; equivalences or other relations, for example. For the purposes of empirical inquiry, aspects of the formal mathematical structure are used to facilitate summarizing and focusing attention on relations among things.

Thus sign process in its evolutionary progress to date may be described as the efforts of man to communicate: first by simple perceptive-manipulative processes; then by verbal processes of increasing complexity, until this increasing complexity of verbal procedure became so much of a barrier to further progress that a shorthand system was devised in order to facilitate further communication. This shorthand system has been most extensively developed in mathematical symboling.

C. LIST OF TRIAL NAMES

Many of the names below were taken from Ch. 11 of Dewey and Bentley's *Knowing and the Known*, while others were used in *A Current Appraisal of the Behavioral Sciences*. The importance of the names does not stem from their sources, but rather from their aid in facilitating communication. The names below are provisionally claimed to be important in the sense that we found them useful in trying to communicate more successfully among ourselves. (In some instances names are listed because we found them to be barriers to mutual understanding.) However, other names overlooked by us may prove to be even more useful than those we here discuss, and some of those presently regarded as useful may prove to be grossly misleading on further inquiry.

A final suggestion to the reader: The prevalence of interactional and self-actional theoretical assumptions may make transactional procedures unfamiliar at first sight. With reference to nomenclature, what seems obvious in self-actional or interactional terms frequently is deficient from a transactional point of view.[1] What may seem odd, peculiar, or overly simple—judged in terms of an acceptance of other procedures—becomes useful, appropriate, and sometimes necessary, given transactional procedures.

For example, Dewey and Bentley have been severely criticized for neglecting what the critics regard as obvious and necessary for all work in the field: distinguishing radically between psychology and logic. Their reply follows:

> "We may assure all such critics that from early youth we have been aware of an *academic* and *pedagogical* distinction of logical from psychological. We certainly make no attempt to deny it, and we do not disregard it. Quite the contrary. Facing this distinction in the presence of actual life processes and behaviors of human beings, we deny any rigid *factual* difference such as the academic treatment implies. . . .We have as strong an objection to the assumption of a science of psychology severed from a logic and yet held basic to that logic, as we have to a logic severed from a psychology and proclaimed as if it existed in a realm of its own where it regards itself as basic to the psychology. *We regard knowings and reasonings and mathematical and scientific adventurings even up to their highest abstractions, as activities of men—as veritably men's behaviors*—and we regard the study of these particular knowing behaviors as lying within the general field of behavioral inquiry. . . ."(*K&K*, p. 180; emphasis in last sentence not in original.)

Note: In the entries below, some quotations are taken from *Knowing and the Known*, Ch. 11. Unless otherwise indicated, we agree with the material quoted.

ACCURATE: Dewey and Bentley suggest this adjective to "characterize degrees of achievement" in the range of specification. However, "degrees of achievement" seems to imply some standards of comparison; standards that we do not have. We suggest that names in the range of scientific specification may be more or less accurate in the sense of more or less painstakingly chosen and applied. Perhaps Dewey and Bentley were naming the same characteristics of naming behavior by their phrase "degrees of achievement." We suggest that "accurate" be used as a short name for "to date found most useful scientifically or by scientists." See PRECISE.

ACTION, ACTIVITY: These words are used here only to characterize loosely durational-extensional subject matters of inquiry. The words suggest self-actional or interactional assumptions in which actions are the doings of independent selves, minds, etc., separated from the full organism-environmental transaction; procedures that are rejected here for inquiry into knowings-known. See INTERACTION; SELF-ACTION; BEHAVIOR.

ACTOR: A confusing although widely used word. "Actor" often is used in ways that unfortunately separate the doer too sharply from the complex behavioral transaction. "Actor" here is used only in the sense of "Trans-actor," the human aspect of a behavioral situation.

APPLICATION: In the terminology adopted here, a name is said to be *applied* to the thing named. Use of "application" helps to avoid the connotation of some intrinsic or necessary relation between the thing named and the marks or sounds used in naming.

ASPECT: The name for any differentiated part of a full transaction, without special durational stress. (For the latter see PHASE.) The aspects are not taken as independent "reals." In a borrower-lender transaction, the borrower, the lender, and what is lent are among the aspects of the transaction. Those aspects are inseparable in that there is no borrowing without lending, and vice-versa.

BEHAVIOR: The name here covers all the adjustmental processes of organism-in-environment. This differs from other uses that limit "behavior" to the muscular and glandular actions of organisms in "purposive" processes, or to the "external" rather than "internal" processes of the organism. "Behavior" here is always used transactionally, never as of the organism alone, but instead as of the organism-environmental process. (This is not to deny that *provisional* separation of organism and environment, within a transactional framework, can be useful in inquiry.)

BEHAVIORISM: Although many conflicting behaviorist procedures of inquiry can be found, a common feature is the rejection of traditional mentalistic and nonscientific procedures. We agree that the latter should be rejected. However, care should be taken to distinguish our transactional procedures from many types of behaviorism, because some behaviorists regard behavior as occurring strictly within the organism or regard behavior as physiological. Our rejection of traditional presuppositions should not be understood as implying exclusion of physiological processes; we include them as aspects of sign behavior. (See SIGN BEHAVIOR; TRANSACTION.)

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[1] The prevalence of nontransactional behavior in inquiry reflects linguistic habits not easily changed. For example, although the authors of *A Current Appraisal of the Behavioral Sciences* adopted a transactional method, in the first edition they sometimes inadvertently separated "internal"—"external," "individual"—"social," "organism"—"environment," and a word from its so-called "meaning," with resulting incoherence. The discussion in the glossary section of the present report suggests the dangers of fusing "biological" and "physiological," and helps to point out the lack of clarity in some of the uses of "operational" and "specification."

BIOLOGICAL: The name given here to those processes in living organisms that are not currently explorable by the techniques of the physical sciences alone. Biological inquiry covers inquiry into both physiological and sign-behavior. No ultimate separation between the physical and biological "realms" is assumed, nor do we assume that present physical and physiological techniques of inquiry will remain unchanged. Perhaps future inquiry will make our present divisions of subject matters unsuitable. See PHYSICAL.

CHARACTERIZING: This name is applied to the everyday use of words that is reasonably adequate for many practical purposes. Characterizing is more advanced than cueing, but less advanced than specifying.

CIRCULARITY: In self-actional and interactional procedures, circularity may constitute a grievous fault. In explicitly transactional inquiry, some circularity is to be expected. For example, the description of useful procedures of inquiry is based on the observation of past successful inquiries; that description in turn may help to improve future inquiries; which in turn may lead to an improved description of procedures; etc. Some critics of Dewey and Bentley regard the type of circularity found in *Knowing and the Known* as a major flaw, but they apparently fail to grasp the significance of the Dewey-Bentley procedures.

COHERENCE: The word is applied by us not to the internal consistency of a set of symbols, but to the connection found in scientific inquiry to obtain between or among objects. Not logical connection, then, but the kind of "hanging together" that occurs in observed regularities, is what is named.

CONCEPT, CONCEPTION: "Concept" is used in so many ways, especially in mentalistic and hypostatized forms, and in ways separating the sign from the sign-user, that its total avoidance is here recommended. "Conception" is frequently construed as a "mentalistic entity," but sometimes as a synonym for a point of view provisionally held and to be inquired into. Even in the latter instance, the word may have mentalistic connotations. We are convinced that it is not useful because it so often is a semantic trap for the unwary.

CONJECTURE: When description is blocked in inquiry, the inquirer imagines what may be happening; "conjecture" designates such a tentative notion about possible connections among facts. In view of the other applications found for "hypothesis," we suggest "conjecture" as a replacement for that name. See HYPOTHESIS, THEORY.

CONNECTION: In naming-knowing transactions, the general name for the linkages among the aspects of a process, as found through inquiry. In an observed regularity, the things involved in the regularity are said to be connected. "Connection" covers the relations sometimes referred to as "causal," "statistical," "probabilistic," "structural-functional," etc.

CONSCIOUSNESS: Not used by us unless as a synonym for "awareness".

CONSISTENCY: Discourse found to be free of contradictory and of contrary assertions is characterized as consistent.

CONTEXT: Here used transactionally to refer to the mutually related circumstances and conditions under which things (objects and events) are observed.

COSMOS: Names the sum total of things we can see, smell, taste, hear, and feel (often aided by instruments), including connections among those things. "Cosmos" is applied to the universe as a whole system, including the speaking-naming thing who uses the name "cosmos." Observable durations extend across cultures, backward into the historical-geological record, and forward into indefinite futures as subject matters of inquiry. Not to be construed as something underlying knowing-knowns yet itself unknowable.

CUE: The earliest stage of designating or naming in the evolutionary scale. Primitive naming, here called "cueing," is close to signaling, and no clear line of demarcation between them is found. The differentiation is made on the basis that organized language occurs in cueing. Some psychologists apply "cue" to what we name "signal," and vice-versa. If such psychological use develops firmly, our use will be superseded.

DEFINITION: Often used in a broad sense to cover any procedure for indicating the "meaning" of a term, including: the stipulation of the application of a term in technical contexts (as when "ohm" is chosen as the name for a unit of electrical resistance); descriptions of the uses a term has in everyday speech; equations relating a single symbol and a combination of symbols for which the single symbol is an abbreviation (as in symbolic logic); what is here called "specifying"; as well as many other procedures. Also used to refer to a description of the "nature" or "essence" of a thing. In view of the many widely varying procedures to which "definition" has been applied, we avoid the term here. See SPECIFYING.

DESCRIPTION: Expansion of naming or designating in order to communicate about things (including situations, events, objects, and relations) on which attention is focused.

DESIGNATING: Always considered here transactionally as behavior. Includes cueing, characterizing, and specifying. When naming and named are viewed in common process, "designating" refers to the naming aspect of the transaction. Designating is the knowing-naming *aspect* of fact.

ENTITY: Its use often presupposes self-actional or interactional procedures, and especially some independent-of-all-else kind of existence. Not used here. See THING.

ENVIRONMENT: *Not* considered here as something surrounding, and fully separable from organisms; but as one aspect of organism-environmental transactions. The apparently plausible separation of organism from environment breaks down when one attempts to locate and consistently describe the exact demarcation between organism and environment. For some purposes of inquiry, focusing attention primarily on either the organic or the environmental aspect of the whole transaction may be useful.

EPISTEMOLOGICAL: To the extent the use of "epistemological" supposes that knowers and knowns are fully separable, the word is incompatible with transactional procedures and is not used here.

EVENT: The name chosen here for durational changes among facts upon which attention is focused for purposes of inquiry.

EXACT: See PRECISE, ACCURATE.

EXCITATION: To be used in reference to physiological organism-environmental processes when differentiation between such physiological stimulation and sign-behavioral stimulation is desired. See STIMULUS.

EXISTENCE: The known-named aspect of fact. Physical, physiological, and behavioral subject matters are regarded here as equally existing. However, "existence" should not be considered as referring to any "reality" supposedly supporting the known but itself unknowable.

EXPERIENCE: "This word has two radically opposed uses in current discussion. These overlap and shift so as to caust continual confusion and unintentional misrepresentation. One stands for short extensive-durational process, an extreme form of which is identification of an isolated sensory event or 'sensation' as an ultimate unit of inquiry. The other covers the entire spatially extensive, temporally durational application; and here it is a counterpart for the word 'cosmos'." "Experience" sometimes is used to name something considered to be primarily localized in the organism ("he experienced delight") or to what includes much beyond the organism ("the experience of the nation at war"); to relatively short durational-extensional processes ("he experienced a twinge") and to relatively vast processes ("the experience of the race"). "The word 'experience' should be dropped entirely from discussion unless held strictly to a single definite use: that, namely, of calling attention to the fact that *Existence* has organism and environment as its aspects, and cannot be identified with either as an independent isolate." See BEHAVIOR.

FACT: The cosmos in course of being known through naming by organisms, themselves being always among its aspects. Fact is the general name for bits and pieces of cosmos as known through naming, in a statement sufficiently developed to exhibit temporal and spatial localizations. Man is included among the aspects of cosmos. "It is knowings-knowns, durationally and extensionally spread; not what is known to and named by any one organism in any passing moment, nor to any one organism in its lifetime. Fact is under way among organisms advancing in a cosmos, itself under advance as known. The word 'fact,' etymologically from *factum*, something done, with its temporal implications, is much better fitted for the broad use here suggested than for either of its extreme and less common, though more pretentious applications: on the one hand for an independent 'real'; on the other for a 'mentally' endorsed report."

FIELD: "On physical analogies this word should have important application in behavioral inquiry. The physicist's uses, however, are still undergoing reconstructions, and the definite correspondence needed for behavioral application can not be established. Too many current projects for the use of the word have been parasitic. Thorough transactional studies of behaviors on their own account are needed to establish behavioral field in its own right." "Field" here names a cluster of connected facts as found in inquiry. We do not use "field" as the name for a presumed separate environment in which independent facts are found; "field" names the entire complex process of mutually connected things and their relations on which attention is focused, and includes the observer in the transaction.

FIRM: Namings are firm to the extent that they are found to be useful for consistent and coherent communication about things, including events. Firmness, thus demonstrated, involves no implication of finality or of immunity to being superseded as scientific inquiry advances.

HUMAN: The word used to differentiate ourselves, our ancestors, and our progeny from the remainder of the cosmos. No ultimate division of the cosmos into man, other organisms, and physical objects is intended. Nor, obviously, do we intend by our naming to deny man's evolutionary development from other organisms, or the myriad connections man has with other aspects of the cosmos.

HYPOTHESIS: In the literature on methodology, "hypothesis" sometimes is applied to any conjecture about possible connections among facts, but sometimes is restricted to relatively exact formulations that may emerge in an advanced stage of inquiry. Sometimes "hypothesis" is embedded in the terminology of traditional logic and epistemology, as when a hypothesis is said to be a proposition not known to be true or false initially, but from which consequences are deduced; if sufficient deductions are confirmed, the "hypothesis" is said to become a "truth." To avoid confusion, we suggest replacing "hypothesis" by "conjecture." See CONJECTURE, THEORY.

IDEA, IDEAL: "Underlying differences of employment are so many and wide that, where these words are used, it should be made clear whether they are used behaviorally or as names of presumed existences taken to be strictly mental." "Idea" may be serviceable as referring to a notion about things.

INDIVIDUAL: "Abandonment of this word and of all substitutes for it seems essential wherever a positive *general theory* is undertaken or planned. Minor specialized studies in individualized phrasing should expressly name the limits of the application of the word, and beyond that should hold themselves firmly within such limits." In the transactional framework here adopted, "behavior" covers both so-called "individual" and "social" behavior, which are aspects of behavioral transactions. See BEHAVIOR.

INQUIRY: "A strictly transactional name. It is an equivalent of knowing, but preferable as a name because of its freedom from 'mentalistic' associations." Scientific inquiry is the attempt to develop ever more accurate descriptions (including what are often called "explanations") of the things and their relations that are differentiated in cosmos, in order to facilitate prediction and control (or adjustive behavior thereto). Statements about the observed regularities, measurements of change, etc., are formulated as warranted assertions.

INTER: "This prefix has two sets of applications (see Oxford Dictionary). One is for 'between,' 'in-between,' or 'between the parts of.' The other is for 'mutually,' 'reciprocally.' " (E.g., this prefix sometimes is applied to the relation "in-between," as when mind and body are said to interact in the pineal gland, or that a tennis ball is intermediate in size between a golf ball and a soft ball. Sometimes "inter" is used for mutually reciprocal relations, as in the interaction of borrower and lender.) "The result of this shifting use as it enters philosophy, logic, and psychology, no matter how inadvertent, is ambiguity and undependability." The habit of mingling without clarification the two sets of implications is easily acquired; we use "inter" for instances in which the "in-between" sense is dominant, and the prefix "trans" is used where mutually reciprocal influence is included.

INTERACTION: "This word, because of its prefix, is undoubtedly the source of much of the more serious difficulty in discussion at the present time." Some authors use "interaction" in the way "transaction" is used here. We restrict "interaction" to instances in which presumptively independent things are balanced against each other in causal interconnection, as in Newtonian mechanics. For inquiry into knowing-knowns, such an interactional procedure is rejected. See TRANSACTION.

KNOWINGS: Organic aspects of transactionally observed behaviors. Here considered in the familiar central range of namings-knowings.

KNOWLEDGE: "In current employment this word is too wide and vague to be a *name* of anything in particular. The butterfly 'knows' how to mate, presumably without learning; the dog 'knows' its master through learning; man 'knows' through learning how to do an immense number of things in the way of arts or abilities; he also 'knows' physics, and 'knows' mathematics; he knows *that*, *what*, and *how*. It should require only a moderate acquaintance with philosophical literature to observe that the vagueness and ambiguity of the word 'knowledge' accounts for a large number of the traditional 'problems' called *the problem of knowledge*. The issues that must be faced before firm use is gained are: Does the word 'knowledge' indicate something the organism possesses or produces? Or does it indicate something the organism confronts or with which it comes into contact? Can either of these viewpoints be coherently maintained? If not, what change in preliminary description must be sought?" See WARRANTED ASSERTION.

KNOWNS: "Known" refers to one aspect of transactionally observed behaviors, i.e., to what is named. "In the case of namings-knowings the range of the knowns is that of existence within fact or cosmos, not in a limitation to the recognized affirmations of the moment, but in process of advance in long durations."

LANGUAGE: Here viewed transactionally as behavior of men (with the possibility open that inquiry may show that other organisms also exhibit language behavior). Word-users here are not split from word-meanings, nor word-meanings from words.

MANIPULATION: See PERCEPTION-MANIPULATION.

MATHEMATICS: Here regarded as a behavior developing out of naming activities and specializing in symboling, or shorthand naming. See SYMBOLING.

MATTER, MATERIAL: See PHYSICAL. If the word "mental" is dropped, the word "material" (in the sense of matter as opposed to mind) falls out also.

MEANING: Not used here, because of confusion engendered by past and current uses. Transactional procedures of inquiry reject the split between bodies-devoid-of-meaning and disembodied meanings.

MENTAL: Not used here. Its use typically reflects the hypostatization of one aspect of sign behavior.

NAME, NAMING, NAMED: Naming is here regarded as a form of knowing. Names are *not* considered here as third things separate from and intermediate between the organism and its environment. Naming transactions are language behavior in its central ranges. Naming behavior states, selects, identifies, orders, systematizes, etc. We at times use "designating" as a synonym for "naming."

OBJECT: Within fact, and within its existential phase, object is that which has been most firmly specified, and is thus distinguished from situation and event. Object is an aspect of situation inquired into insofar as useful description or firm naming of that aspect has been achieved.

OBJECTIVE: Used here only in the sense of "impartial" or "unbiased."

OBSERVATION: Used here transactionally, rather than as a separated "activity" of the observer. Observation and reports upon it are regarded as tentative and hypothetical. Observation is not limited to "sense-perception" in the narrow sense; i.e., to a "simple" sensory quality or some other supposed "content" of such short time-span as to have no or few connections. Observation refers to what is accessible and attainable publicly. Both knowings and electrons, for example, are taken as being as observable as trees or chairs.

OPERATION: "The word 'operation' as applied to behavior in recent methodological discussions should be thoroughly overhauled and given the full transactional status that such words as 'process' and 'activity' require. The military use of the word is suggestive of the way to deal with it."

OPERATIONISM: This has become a confusing word, and sometimes seems to be merely an invocation of scientific virtue. "Operational definition" sometimes refers to defining phrases having an "if–then" form ("x is water soluble"="if x is immersed in water, then it dissolves"); sometimes to the insistence that the criteria of application of a word be expressed in terms of experimental procedures; and sometimes to a statement of the observable objects and events that are covered in the use of a word. On some occasions, "operational definition" apparently is used to refer to something similar to, if not identical with, what we call "specification" or scientific naming. See SPECIFYING.

ORGANISM: Used here to differentiate living things from other things in the cosmos, but *not* to detach organisms from their many connections with other aspects of cosmos. Organisms are selected for separate naming for methodological purposes, not as constituting something separated from the rest of cosmos.

PERCEPT: In the transactional framework, a percept is regarded as an aspect of signaling behavior, not as a hypostatized independent something.

PERCEPTION-MANIPULATION: Although perception and manipulation are regarded as radically different in some procedures of inquiry, transactionally viewed they have a common behavioral status. They occur jointly and inseparably in the range of what is here called signal behavior.

PHASE: Used for an aspect of cosmos when attention is focused on the duration of a time sequence, as when referring to the various phases of the manufacture and distribution of products.

PHENOMENON: Used here for provisional identification of situations. Not to be construed as "subjective," nor as a mere appearance of an underlying reality.

PHYSICAL: At present, we find three major divisions of subject matter of inquiry: physical, physiological, and sign-behavioral. These divisions are made on the basis of present techniques of inquiry, not on the basis of assumed essential or ontological differences. See BIOLOGICAL.

PHYSIOLOGICAL: "That portion of biological inquiry which forms the second outstanding division of the subjectmatter of all inquiry as at present in process; differentiated from the physical by the techniques of inquiry employed more significantly than by mention of its specialized organic locus." See BEHAVIORISM.

PRECISE: Dewey and Bentley use "exact" as an adjective to describe symbols, and "accurate" to describe specifying. We question the usefulness of differentiating between specifying and symboling other than to point out that the latter seems to be shorthand for the former. Because symbols are often used in connection with relatively precise measurements for the purposes of scientific inquiry, we suggest that "precise" may be more useful than "exact" as an adjective characterizing any symbolizing. Symbols are precise to the extent that they are shorthand names for precise measurements or what could be precise measurements. See ACCURATE.

PROCESS: To be used aspectually or phasally as naming a series of related events.

PROPOSITION: Used sometimes in the context of logic to name the states-of-affairs to which statements (or assertions, or sentences) refer. Thus "The dog is black" and "Der Hund ist schwarz" are said to express the same proposition. Generally such procedures make sharp distinctions among words, word-users, and "meanings," or among namers, nameds, and names. Such separations are here rejected, and along with them go many related distinctions. We regard the talkings (including namings, thinkings, reasonings, etc.) of man as human behavior rather than as third things somehow occurring between men and what they talk about, and we believe that proceeding in this manner not only avoids many needless mysteries but aids scientific inquiry into such talkings.

QUEST FOR CERTAINTY: In prescientific inquiry, the attempt to discover an eternal and immutable "reality" that can be known with complete certainty. We do not assert the absolute nonexistence of such "reality," but point out the failure to find it and the barrier such a notion has been to scientific progress. In somewhat disguised forms, the quest for certainty crops up in purportedly scientific investigations, as in attempts to find a certain and indubitable base upon which inquiry rests.

REACTION: In physiological stimulation (as contrasted with sign-behavioral stimulation), "excitation" and "reaction" are coupled as aspects of the stimulation transaction. See EXCITATION, STIMULUS.

REAL: Used sparingly as a synonym for "genuine," in opposition to "sham" or "counterfeit."

REALITY: "As commonly used, it may rank as the most metaphysical of all words in the most obnoxious sense of metaphysics, since it is supposed to name something which lies underneath and behind all knowing, and yet, as Reality, something incapable of being known in fact and as fact."

RESPONSE: In signaling behavior, as differentiated from physiological stimulation, "stimulus" and "response" are coupled as aspects of the stimulation transaction.

SCIENCE, SCIENTIFIC: "Our use of this word is to designate the most advanced stage of specification of our times—the 'best knowledge' by the tests of employment and indicated growth."

SELF: Within the framework here adopted, "self" names one aspect of organism-environmental transactions, rather than an hypostatized "entity."

SELF-ACTION: "Used to indicate various primitive treatments of the known, prior in historical development to interactional and transactional treatments." That is, used to refer to frameworks in which presumptively independent actors, minds, selves, etc., are viewed as causing events (as, for example, when gods are said to cause meteorological phenomena, or minds to create new ideas). "Rarely found today except in philosophical, logical, epistemological, and a few limited psychological regions of inquiry."

SIGN: The name applied here to organism-environmental transactions in which the organism involved in a situation accepts one thing as a reference or pointing to some other thing. "Sign" here is *not* the name of the thing that is taken as referring to something else; rather "sign" names the whole transaction. The evolutionary stages of "sign" are here named "signal," "name," and "symbol."

SIGNAL: Used here to refer to the perceptive-manipulative stage of sign process in transactions such as beckoning, whistling, frowning, etc. No clear line of demarcation between signaling and cueing is found; some perceptive-manipulative signalings are not only alerting behaviors, but also may begin to describe aspects of cosmos.

SIGN-BEHAVIOR: Sign-behavior refers to that range of biological inquiry in which the processes studied are not currently explorable by physical or physiological techniques alone. Human behavior here covers both so-called "social" and "individual" behavior. No ultimate or ontological separation of physical, physiological, and sign-behavior is assumed; the distinction made here concerns the techniques of inquiry found useful for various types of subject matters. See PHYSICAL, PHYSIOLOGICAL.

SIGN-PROCESS: Synonym for SIGN.

SITUATION: Used here as a blanket name for a limited range of fact, localized in time and space, upon which attention is focused. "In our transactional development, the word is not used in the sense of environment; if so used, it should not be allowed to introduce transactional implications tacitly."

SOCIAL: See INDIVIDUAL.

SPACE-TIME: Space and time are here used transactionally and behaviorally, rather than as fixed, given frames (formal, absolute, or Newtonian) or physical somethings. Bentley's words suggest our present approach: "The behaviors are present events conveying pasts into futures. They cannot be reduced to successions of instants nor to successions of locations. They themselves span extension and duration. The pasts and the futures are rather phases of behavior than its control."[1]

SPECIFYING: Used here to refer to the naming that has been found useful in science. "The most highly perfected naming behavior. Best exhibited in modern science. Requires freedom from the defectively realistic application of the form of syllogism commonly known as Aristotelian." Should *not* be mistaken as a synonym for "definition," at least in many senses of the latter word.

STIMULUS: Used in various ways in current inquiry, sometimes designating an object or group of objects in the environment, sometimes something in the organism (events in the receptors, for example), and sometimes something located elsewhere. The near chaos connected with this word strongly suggests the need for a transactional procedure. "Stimulating" may be a preferable term, inasmuch as it suggests a transactional process.

SUBJECT: Used here in the sense of "topics," as in "subject matter being inquired into," rather than in any sense postulating a radical separation of subject and object.

SUBJECTIVE: The usual subjective-objective dichotomy is rejected here, and what commonly are called "subject" and "object" are regarded as aspects of relevant transactions. However, inasmuch as some inquiries in philosophy and psychology still use procedures based on "subjective" analysis or introspection, we emphasize our objection to whatever is not publicly observable. Subjectivism, understood as a procedure of inquiry attempting to obtain scientifically useful "knowledge" from what is not publicly accessible, is rejected here.

SUBJECT MATTER: "Whatever is before inquiry where inquiry has the range of namings-named. The main divisions in present-day research are into physical, physiological, and behavioral."

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[1] Arthur F. Bentley, *Inquiry Into Inquiries* (Sidney Ratner, ed.) Boston, Beacon Press, 1954, p. 222.

SUBSTANCE: No word of this type has a place in the present system of naming.

SYMBOL: A shorthand naming component of symboling behavior. As used here, not to be hypostatized, but viewed transactionally and comparable with "name" and "signal."

SYMBOLING: Symboling, in scientific inquiry, is a shorthand means of specifying or scientifically naming. In the development of pure mathematics structures, consistency within the symbol system is of primary importance. In such instances the symbols do not directly designate specific things and events but rather designate potential relations. (E.g., "2" does not name the type of thing that "dog" does.) However, when mathematics is used in scientific inquiry, the mathematical symbols are applied to the subject matter; then the symbols become shorthand specifications or abbreviated names.

SYSTEM: Used here as a blanket name to refer to sets or assemblages of things associated together and viewed as a whole. Systems may be self-actional, interactional, or transactional. Typically used here in the transactional sense of "full-system," in which the components or aspects are not viewed as separate things except provisionally and for special purposes other than a full report on the whole situation.

TERM: "This word has today accurate use as a name only in mathematical formulation where, even permitting it several different applications, no confusion results. The phrase 'in terms of' is often convenient and, simply used, is harmless. In the older syllogism term long retained a surface appearance of exactness which it lost when the language-existence issues involved became too prominent. For the most part in current writing it seems to be used loosely for 'word carefully employed.' It is, however, frequently entangled in the difficulties of concept. Given sufficient agreement among workers, term could perhaps be safely used for the range of specification, and this without complications arising from its mathematical uses."

THEORY: Widely used in many differing applications; i.e., as conjecture, notion, hypothesis, final outcome of inquiry, etc. We suggest that "theory" be used to designate the description of what happens under specified circumstances. So used, a theory is highly warranted by the evidence presently available (e.g., the theory of evolution), but is subject to future correction, modification, or abandonment. See DESCRIPTION, WARRANTED ASSERTION.

THING: Used here as the general name for whatever is named. Things include both objects and events; any and every aspect of cosmos.

TIME: See SPACE-TIME.

TRANS: This prefix is used to indicate mutually reciprocal relations. See INTER.

TRANSACTION: Refers here to the full ongoing process in a field. In knowing-naming transactions, the connections among aspects of the field and the inquirer himself are in common process. To be distinguished from "interaction" and "self-action." See INTERACTION and SELF-ACTION.

TRUE, TRUTH: The many conflicting uses of these words incline us not to use them. In their senses of "can be relied upon," "in accordance with states-of-affairs," and "conformable to fact," they name what we call "warranted assertions." However, the connotation of permanence, fixity, and immutability suggests the quest for certainty. See WARRANTED ASSERTION.

VAGUE: This term refers to various types of inaccuracy and imprecision. Probably "vagueness" could profitably be replaced by other words indicating just what type of inaccuracy or imprecision is involved.

WARRANTED ASSERTION: Used here to refer to those assertions best certified by scientific inquiry. Such assertions are open to future correction, modification, and rejection; no finality is attributed to them. See INQUIRY.

WORD: As used here, there is no supposed separation of "meaning" from a physical vehicle somehow carrying that "meaning." Words are viewed transactionally as an aspect of knowing behavior; the subject matter is inquired into whole, as it comes, not as bifurcated.

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